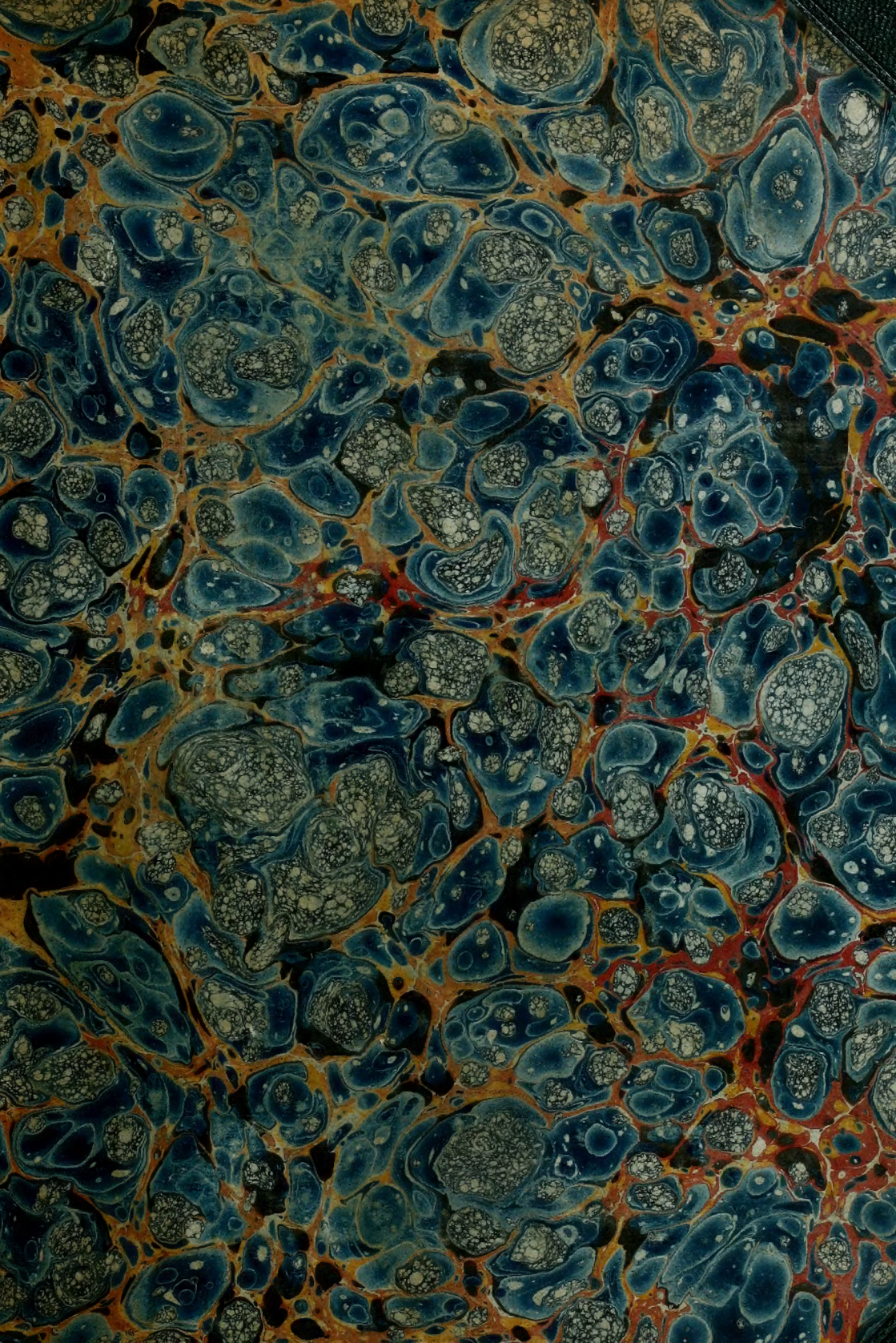

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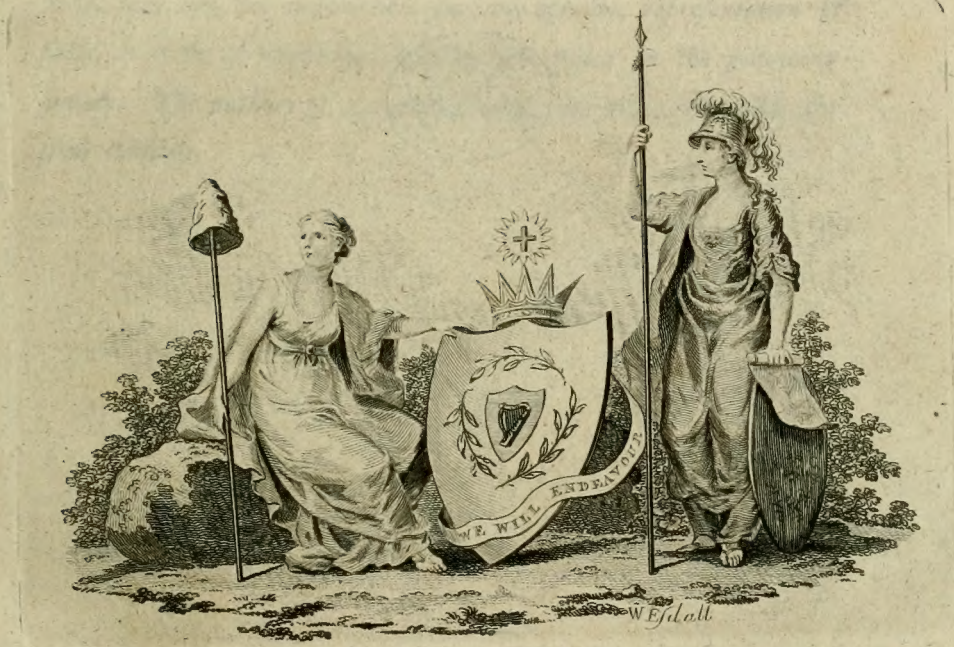
VOL. VIII.



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ERRATA.

Science.

Page 8, Line 15, *for* analagous, *read* analogous.
—— 17, Line 16, *for* of one of ſome, *read* of ſome one.
—— 21, Line penult. *for* analiſts, *read* analyſts.
—— 36, Line penult. *for* Lulolph, *read* Ludolph.
—— 40, Line ult. *between* of *and* greateſt, *inſert* the.
—— 46, Line 12, *for* ſenſe, *read* courſe.
—— 49, Line 10, *for* diſtintigration, *read* diſintegration.
—— 51, Line 7, *between* is *and* the, *inſert* in.
—— 60, Note, *for* Rox, *read* Roz.
—— 63, Line 2, *for the* full ſtop *after* import, *ſubſtitute* a comma.
—— 64, Note, *for* Rox, *read* Roz.
—— 74, Line ult. *for* comſounded *read* conſounded.
—— 112, End of firſt line, *ſupply* it.
—— 136, Line 2, *for* ſtep, *read* ſteps.
—— 155, Line 8, *for* broj, *read* broil.
—— 221, Line 5, from bottom, place a vinculum over the quantity following the ſign $\times$.
—— 226, Line 1, *after* centripetal, *read* force.
—— 229, Line 2, for $n+n$, *read* $n+2$.

Antiquities.

Page 6, Line 17, *for* lies, *read* lying.

SCIENCE.

OBSERVATIONS *on the* PROOFS *of the* HUTTONIAN THEORY *of the* EARTH, *adduced by* SIR JAMES HALL, BART. *By* RICHARD KIRWAN, *Esq*; *L. L. D. F. R. S. and P. R. I. A.*

Read Feb. 8, 1800.

AS ſome poſitions, which I laid down in my examination of Dr. Hutton's theory of the earth, may ſeem queſtionable from the ingenious reaſoning employed by Sir James Hall in the third volume of the Edinburgh tranſactions to corroborate ſome of Dr. Hutton's aſſertions, and may even be thought inconſiſtent with ſome of the curious reſults that occurred in the highly intereſting experiments inſtituted by the worthy Baronet, inſerted in the fifth volume of the Edinburgh tranſactions, (a printed tranſcript of which he has had the goodneſs to ſend me) I think it a duty incumbent upon me to examine both the general reaſoning employed by him, and the conſequences fairly deducible from his experiments; fanciful and groundleſs as the Huttonian theory ſeems to me to be, it may, like the reſearches for the philoſopher's ſtone, be highly uſeful by ſuggeſting new experiments.

In the third volume of the Edinburgh tranſactions hiſt. p. 9, we are informed that Sir James Hall, though convinced from various obſervations that granite had once flowed in a ſtate of fuſion, yet acknowledged that ſome difficulties accompanied this opinion; among which the moſt conſiderable appeared to him to be this, that in ſome caſes the felſpar is ſeen in this ſtone with its cryſtals regularly defined, whereas the quartz forms a confuſed and irregular maſs, being moulded on the cryſtals of felſpar, whereas if the granite were formed by fuſion the very contrary, he ſays, ſhould, it would ſeem, be expected; felſpar being very fuſible, and quartz on the contrary highly infuſible. In anſwer to which he ſays " that when quartz and felſpar are mixed and pounded " together, it is well known they may be melted without diffi- " culty into a kind of glaſs, the felſpar ſerving as a flux to the " quartz; or the felſpar may be conſidered as a menſtruum in " which the quartz is diſſolved; and in this view we may expect " by analogy phænomena ſimilar to thoſe of the ſolution of ſalt " in water. Now it is certain that when exceſſive cold is applied " to ſalt water, the water is frozen to the excluſion of the ſalt; " why ſhould not the ſame thing happen in the ſolution of quartz " in the liquid felſpar, when the maſs is allowed to cool beneath " the point of congelation of the menſtruum? the felſpar may " cryſtalize ſeparately from the quartz, as we have ſeen pure ice " formed ſeparately from the ſalt."

In

In this anſwer ſeveral particulars deſerve conſideration. In the firſt place water (to which felſpar is here aſſimilated) is never regularly cryſtallized when frozen by exceſſive refrigeration, though indeed vapour may; conſequently ſince in the preſent caſe the felſpar is ſaid to be regularly cryſtallized, the parity does not hold.

Again, to juſtify the compariſon of felſpar acting on quartz as a menſtruum, as water does upon ſalt, the felſpar ſhould always be in the larger, and quartz in the ſmaller proportion to each other, as water always is to ſalt, and this is indeed the commoneſt caſe even where the felſpar is not regularly cryſtallized; yet in Switzerland this does not happen, as Mr. Hoepfner atteſts, 4 Helvetic Magaz. p. 266; of which ſpecimens may be met in 2 Loſke Catal, Engliſh edition, p. 375, 376, No. 37, 38, 40, 41, nor in Sileſia, as Gerhard remarks, 1 Grundriſs Min. Syſtem, p. 404 and 405. How then could the felſpar have ſerved as a menſtruum or flux to the quartz in theſe caſes?

It is allowed by all obſervers that the caſes in which felſpar in granite is regularly cryſtallized are exceeding few, ſee Lentz, Emerling, Widenman, &c. Granites in which ſuch cryſtals are obſerved are called *porphyraceous granites*, and from that very circumſtance judged by many obſervers not to be ancient granites, but of modern formation, ſee 2 Widenman, p. 1005 in the note. An obſervation ſimilar to that of Sir James Hall has alſo been made

made by Mr. Beſſon in Limoges, 29 Roz. Tour, p. 89, for he diſcovered veins of granite in an argillite, though this ſchiſt did not border upon any granitic maſs, and hence he judged it of modern formation. Citizen Dolomieu alſo tells us that ſuch inſtances had occurred to him in his travels, but he thinks them perfectly diſtinct from the granite which forms granitic mountains, 16 Journ. des Mines, p. 22. Neither was Sauſſure a ſtranger to ſuch granitic veins, but he accounts for their origin very differently from Sir James, § 600.

Various attempts have been made to fuſe granites, in moſt of which, as has already been ſaid, felſpar is the moſt abundant ingredient, but in almoſt all, though finely pulverized, the quartz remained unfuſed and might be diſtinguiſhed by a lens, ſee 1 Sauſſure, §. 172, 173 and 174, 1 Gerh. Geſch. §. 51, and in the firſt part of his new mineral ſyſtem publiſhed in 1797, p. 412, and Hacquet in 1 Crell Beytrage, p. 34, 35, &c. It is plain then, that in all heats with which we are acquainted the felſpar cannot but in very rare caſes ſerve as a flux or a menſtruum to the quartz with which it is found in granites, the full proportion of quartz which can be rendered fuſible by its other component earths being already contained in the felſpar, and in fact there is no analogy betwixt water acting as a menſtruum on ſalt, and felſpar acting on quartz, for water and ſalt are ſubſtances perfectly heterogeneous to each other, whereas felſpar and quartz are both earthy ſubſtances of which

which the former contains a large proportion of the latter, as eſſential to its compoſition, and is fuſible only by reaſon of its compound nature, but if the quantity of the quartzy ingredient be increaſed the whole becomes infuſible, as I have experienced; whereas if the proportion of ſalt in water be increaſed, ſtill the water will be congealable if conſiderably cooled; moreover quartz frequently bears the impreſſion of ſtones more fuſible than itſelf, which could not happen in any poſſible ſuppoſition, if all had been in a ſtate of fuſion.

Again, Sir James obſerved, that a quantity of green glaſs, which had been allowed to cool ſlowly, was found to have loſt all its vitreous properties, being opake, white and refractory; but being again melted by a blow-pipe and ſuddenly cooled, it reſumed its former properties and became glaſs; hence he infers that if the glaſs produced by the fuſion of granite had been allowed to cool with ſufficient ſlowneſs, it might have cryſtallized, producing a granite ſimilar to the original, p. 11.

The obſervation on glaſs here mentioned is perfectly juſt and has been often repeated; but the analogy betwixt this caſe and the formation of granite from a complete fuſion of its ingredients is far from being accurate. Glaſs conſiſts of a ſimple earth, namely, the ſiliceous united to an alkali. To form this union it is neceſſary that the integrant affinity of the ſiliceous particles to each other ſhould yield to the chymical affinity which

which the alkali bears to them, and this can happen only in ſo high a degree of heat as conſiderably leſſens the affinity of the ſiliceous particles to each other. If, when this union is effected, the compound is conſiderably and *rapidly* cooled, yet the union will ſtill continue, becauſe the alkaline menſtruum being congealed, the ſiliceous particles cannot move through it to reunite to each other, though their affinity to each other in a *low* temperature be greater than their affinity to an alkali, and thus they continue in that ſtate which we call *glaſs*. Two experiments ſet this explication beyond all doubt, the firſt is, that if a ſolution of ſalt in water be ſuddenly cooled from one hundred and forty degrees above to ſix degrees below o of Fahrenheit, the whole will be congealed, and no ſeparation of the ſalt will take place, ſee 8 Nov. Comment. Petropol. p. 346. This caſe is perfectly analagous to that of glaſs. The ſecond experiment is that of Tromſdorf, 22 An. Chym. p. 115, where we find the ſiliceous particles to have ſeparated by long ſtanding (eight years) from the alkaline in a ſolution of ſilicited alkali, and to have formed perfect cryſtals hard enough to ſtrike fire with ſteel.

That the glaſs thus formed, being ſuffered to cool ſlowly, ſhould be decompoſed is very natural; it is what happens when certain ſalts, for inſtance nitre, are diſſolved in water to ſaturation, in a boiling heat; if the water be ſlowly cooled moſt of the nitre will cryſtallize and ſeparate itſelf. That the ſiliceous earth

earth thus ſeparated ſhould be more refractory than before, ſhould be alſo expected; both becauſe it is not repulverized (at leaſt not ſtated to have been ſo) and becauſe much of the alkali, which is its menſtruum, evaporates and is volatilized during the ſlow refrigeration. But if the heat applied be much greater than at firſt, it may be vitrified a ſecond time, as more of moſt ſalts may be diſſolved in a ſmall quantity of water at 212° than at 150°.

But to reproduce granite from a general fuſion of all its ingredients by a refrigeration ever ſo ſlow, is a very different caſe from that we have juſt conſidered.

Granite is an aggregate ſtone conſiſting of quartz, felſpar and mica; of theſe the moſt fuſible is undoubtedly the felſpar, and the quartz the leaſt; let us then to indulge the worthy Baronet ſuppoſe all three in perfect fuſion in a high degree of heat, and afterwards ſlowly cooled, and thus each (though vouched by no experiment) gradually reproduced; the quartz, with the exception of the proportion thereof which enters into the compoſition of felſpar and of the mica would undoubtedly cryſtallize firſt on the ſmalleſt diminution of heat, and, being congealed in a medium ſtill in a liquid ſtate, I do not ſee why it ſhould not form regular cryſtals, which nevertheleſs ſcarce ever occur in granite except in cavities. Over this, and after a conſiderable interval of time, the mica ſhould alſo be regularly cryſtallized,

and laſt of all, the felſpar ſhould coaleſce and congeal, (at leaſt in the Baronet's ſuppoſition) in regular cryſtals; now as the cryſtallizations of theſe three ſpecies of ſtone take place each at a diſtinct portion of time, each ſhould occupy alſo a diſtinct portion of ſpace, the firſt ſet of cryſtals being loweſt, the next over that and the laſt uppermoſt, as we find to happen when ſalts of very different ſolubility and yet in equal quantity are diſſolved and cryſtallized in water, or when ſubſtances of different degrees of volatility are ſublimed by fire. Now, among the immenſe maſſes of granite that have been obſerved and examined in various parts of the globe, not above half a dozen have occurred in which the three conſtituent parts of granite were regularly cryſtallized, very few in which diſtinct layers of each were ſeen, and *none at all* conſiſting of diſtinct regular cryſtals of each, ſuperimpoſed on each other. On the contrary in far the greater number of granitic maſſes the three above named conſtituent maſſes lie intermixed with each other in the moſt confuſed and irregular manner, and without any appearance of regular cryſtallization; inſomuch that none can ſay, from bare inſpection only, which was cryſtalized firſt, and which laſt.—nay granitic maſſes not unfrequently occur, in which it is evident that the mica muſt have cryſtallized contemporaneouſly with the quartz, for in breaking the quartzy part flakes of mica are found within it. See 6, Sauſs. § 1621.

Lastly, I muſt add, that even on the ſuppoſition that diſtinct cryſtals of quartz, felſpar and mica could be produced by fuſion,

fuſion, they ſtill would be far from reſembling thoſe we are acquainted with, which eſſentially contain ſome particles of water, as I have elſewhere ſhewn.

Perhaps ſome may ſay that the ſame difficulties occur in accounting for the cryſtallization of granite in the moiſt way; on mature conſideration however it will readily be ſeen, that the cauſes of coadunition in the dry and the moiſt way are very different, and that their effects ſhould alſo be different. For ſuppoſing the earths, that enter into the compoſition of granites, diſſolved in the moiſt way, their precipitation and imperfect cryſtallization may be aſcribed to the union they contract with each other forming maſſes of each of the conſtituent ingredients of granite, which water can no longer hold ſuſpended; hence the precipitation of each of the three ſpecies of ſtone is nearly *contemporaneous*, whereas if the formation of theſe ingredients ſhould take place in the dry way, it would neceſſarily be *ſucceſſive*, keeping pace with the ſucceſſive diminutions of heat, and then the abovementioned conſequences would naturally enſue.

The ſtate of the granitic ingredients in fuſion which I have above given agrees pretty nearly with that preſented by Sir James himſelf; he ſuppoſes the quartz, felſpar, ſhorl, mica, garnet, &c. melted together, and the moſt fuſible of them to be the menſtruum in which the reſt are diſſolved, and that they differ from each other in their properties of ſolution as ſalts

differ from each other. Some of them being more ſoluble in the menſtruum when very much heated than when it is comparatively cold, and others may be ſoluble in it when little warmer than its point of congelation.——" If then we ſay, for inſtance, " that the congealing point of the ſolvent is 1000° of Fahrenheit " and if the ſolution is at the temperature of 2000°, we may " conceive one portion of the matters diſſolved as held by the " ſimple diſſolving power of the menſtruum, and another as " held by means of its elevated temperature; when therefore " a maſs of this kind is allowed to cool very ſlowly thoſe ſub- " ſtances held in ſolution by the heat of the ſolvent will firſt " ſeparate, and being formed in a liquid will aſſume their cryſ- " taline form with perfect regularity."—This conſequence is truly deduced from the Baronet's hypotheſis, but being contrary to fact diſcovers the falſhood of that hypotheſis, for if any of the fore-mentioned component parts of granite can be ſaid to be held in ſolution by the high heat of the ſolvent, it is ſurely the quartz; now the quartz is ſcarce ever found regularly cryſtallized when forming a component part of granite, as all mineralogiſts atteſt and is matter of univerſal obſervation.

But the Baronet continues, " whereas thoſe ſubſtances which " were held by the menſtruum ſimply as a fluid, will not ſepa- " rate until the congelation of the ſolvent itſelf takes place, " when the cryſtals of the various ſubſtances will intermix and " confound

" confound the regularity of form which each would have af-
" ſumed if left to itſelf. In this manner one of the common
" kinds of granite will be produced conſiſting of perfect cryſtals
" of ſhorl, mica or garnet incloſed in a confuſed maſs of felſpar,
" quartz and ſhorl."—This concluſion is as objectionable as the foregoing; for not to mention that granites, in which ſhorl and eſpecially garnets are found, are far from being common, aſſuredly ſhorl and garnet approach more to the fuſibility of felſpar (the ſuppoſed menſtruum) then either quartz or mica. Theſe therefore are thoſe which ſhould cryſtallize without any regular form in the Baronet's hypotheſis, and not the quartz and mica; which is juſt the contrary of what he himſelf has obſerved, for he tells us, p. 9, " he found the cryſtals of felſpar regularly " defined."

Sir James has ſince very wiſely declined juſtifying his theory f the formation of granite by fuſion, and by the advice of octor Hope very properly applied himſelf to experiments on rious ſpecies of *whin*, a denomination which in Scotland comphends grunſtein, baſalt, trap, wacken and porphyry, ſtones inwhich, except the laſt, none of the component ingredients arfound regularly cryſtallized, and on the laſt he has made no expriment.—The former he tells us were ſoftened or fuſed in a heaof from 38° to 55° of Wedgwood, the glaſſes to which they werereduced were ſoftened on a range of from 15° to 24, and the

the maſſes of the original ſtony appearance, to which thoſe glaſſes were reduced by ſlow cooling, were ſoftened in degrees of heat of from 32° to 45°; to the formation of theſe laſt he conſtantly applies the term *cryſtallization* and calls them *cryſtalites*. To the vague term of cryſtallization I muſt however object, for as thoſe ſtones in their original ſtate preſent no regular cryſtals, but are at moſt internally and imperfectly cryſtallized, ſo they muſt be when reduced from a glaſſy ſtate to one reſembling their original and thus diſcover rather a *niſus* towards cryſtalization than perfect cryſtals, which latter the term cryſtallization generally applied would lead us to expect.

Before I proceed to the detail of theſe experiments, I muſt obſerve that the different fuſibilities of theſe *cryſtalites*, as he calls them, indicate a very different ſtate from that in which they originally exiſted; the former requiring a heat of from 32° 45' and the latter a heat of from 38° to 55°, the reaſon of whic is eaſily diſcovered when the two ſtates are deduced from a different origination, but is in vain ſought for, when both are o be deduced from one and the ſame origin.

Passing over the general preliminary accounts of theſe experiments, which are to be found from p. 7 to p. 10 of this diſſertation, I ſhall now examine the moſt important particulars of each, as far as they give occaſion to any ſtriking obſervations. In this examination I am much aſſiſted by the ingenious accurate

rate and ſkilfully conducted analyſis of Doctor Kennedy, who bids fair to rival the excellence attained by the greateſt maſters of that ſublime and difficult art.

Experiment the firſt.

Grunſtein a compound of felſpar and hornblende intimately mixed with each other, was the ſubject of this experiment. Its *colour*, black or greeniſh black intermixed with pale reddiſh brown; both the felſpar and hornblende imperfectly and confuſedly cryſtallized in minute grains; the *fracture* partly ſtriated and partly foliated. *Luſtre* moderate, its hardneſs 7, or almoſt 8, gives an earthy ſmell when breathed upon, and frequently contains ſmall ſpecks of pyrites, ibid. p. 7.

This ſubſtance he vitrified by a ſtrong heat and ſubſequent rapid cooling, p. 9. A fragment of the glaſs thus produced being introduced under a narrow muffle and heated to 21° in one minute became ſo ſoft as to yield readily to the preſſure of an iron rod, but after a ſecond minute it became quite hard though the temperature had been ſtationary; the ſubſtance thus hardened underwent a thorough change, it loſt its vitreous character, its fracture was like that of *porcelain* (that is even) and it was fuſible only in a heat of 31° p. 11. In another experiment, (ibid.) he found this change to take place even before the glaſs was in perfect fuſion; for while both ends of a fragment of this glaſs were ſupported

ſupported on reſts of clay, it was found not to ſink down between them until the heat was raiſed to 30°—In another experiment he found the conſolidation which he (improperly as I think) calls cryſtallization, to take place even while the heat was gradually increaſed, and the ſubſtance ſtill ſo viſcid as to retain the original ſhape of the fragments.—In another experiment where the glaſs was ſlowly cooled its texture was found completely to reſemble that of whinſtone, the *fracture* was *rough*, *ſtony* and *cryſtalline*, with a number of ſhining facettes interſperſed through the maſs and a few cryſtals in the cavities produced by air bubbles, p. 8.

These experiments may be conſidered in two points of view, firſt with reſpect to phænomena of conſolidation in a heat either gradually increaſed *above* or gradually diminiſhed *below* the heat neceſſary to ſoften the vitreous ſubſtance, the loſs of the vitreous character and the *ſtony* appearance aſſumed through ſlow refrigeration.

And in the ſecond place we may examine how far the phænomena here obſerved tend to countenance the Huttonian theory either of the formation of granite, trap or baſalt or other ſtony ſubſtances.

In this reſpect only it concerns me to examine theſe experiments, yet I cannot forbear mentioning ſome few reflections on the firſt.

It has been obſerved by all thoſe who have attended to the formation of common glaſs (and is indeed evident from the fumes that float over its ſurface) that from the inſtant it enters into fuſion, it is in a conſtant ſtate of decompoſition, gradually becomes leſs fuſible and increaſes in denſity; the ſubſtances that thus eſcape are, in this caſe the ſaline, as Boſc D'Antic has ſhewn and Maquer alſo aſſerts, (See 1 Boſc D'Antic, 10 and 242, 213.) and hence the loſs of weight which glaſs thus ſuffers, ibid. 220, and 4 Maquer, 261; Maquer alſo obſerved that glaſs kept too long in fuſion looſes its tranſparency and becomes opake, becauſe the flux evaporates; and he obſerves that glaſſes formed of argil, lime and gypſum, are particularly ſubject to this accident. Lavoiſier noticed the ſame phænomenon during the fuſion of felſpar even by oxygen air, namely, that the longer it was kept in fuſion the more infuſible it became, Mem Par. 1783, p. 577, which he imputed to the volatility of one of ſome or other of its ingredients; and he afterwards found occaſion to extend the ſame remark to ſteatites and alſo to a mixture of equal parts of quartz and calcareous ſpar; this increaſed infuſibility of certain ſubſtances by a gradually increaſed or continued heat is not therefore a new diſcovery, having been already noticed; but Sir James Hall has conſiderably inlarged it, by ſhewing that the ſtones he operated upon had reaſſumed their ſtony appearance, after having been in a vitreous ſtate; this appearance, if I under-

ſtand him rightly, they have aſſumed only in conſequence of ſlow cooling, and not merely by a heat either ſtationary or gradually increaſed; conſolidation only being the effect of ſuch treatment.

This conſolidation, Sir James calls *cryſtallization*, a term which ſeems to me highly improper; for according to every ſenſe in which this term has ever been employed, whether that operation was perfect or confuſed, it denotes at leaſt an union of particles previouſly diſperſed through a liquid medium; they muſt therefore be at liberty to move through this medium in order to coaleſce and re-unite to each other; if both they and the medium itſelf coaleſce and conſolidate, this action is called *coagulation*, as happens in what was called the *offa helmontii* and the jelly formed by the *liquor ſilicum*; but in Sir James's experiment we find the conſolidation to take place in a fragment of glaſs, which ſtill retained its ſolid ſtate, and conſequently the particles were not at liberty to move towards each other; this conſolidation muſt therefore evidently have ariſen from ſome internal change in the conſtitution of the glaſſes in which it was obſerved; what theſe changes may have been I ſhall now examine: In the firſt place it is highly probable that ſilex, argil and lime, and ſlightly oxygenated calx of iron, whatever be their affinity to each other when duly proportioned, require like all ſolids to abſorb in their paſſage to a liquid ſtate a certain portion of latent heat; but when

when in fuſion and the particles of each chymically united, they require a higher degree of heat to keep them in fuſion, their elective affinities promoting fuſion before the union, and impeding it after the union is formed; it is thus that iron and platina, metals ſeparately highly infuſible, contribute to each others fuſion; but when fuſed, become ſtill more infuſible, as appears by Rinm. § 135; ſulphur and lead are ſeparately and eaſily fuſible, but when united their fuſion becomes much more difficult.

Again, Dr. Kennedy has diſcovered that all theſe whins contain ten per cent. of ſoda, and Vauquelin has lately diſcovered tartarin in felſpar; in the high heats to which theſe ſtones are expoſed in order to vitrify them, may it not be ſuppoſed that theſe ſalts are in ſome meaſure volatilized and the compound thus rendered leſs fuſible? Though in an high heat rapidly produced, they may ſtill be fuſible, as a ſmaller proportion of ſoda will in that circumſtance ſuffice to that effect.

The next circumſtance to be accounted for is the ſaxification or *ſtony* appearance aſſumed by the vitrified ſtones when ſlowly cooled, by far the moſt curious fact, for which we are indebted to the ingenuity of Sir James. To account for this change it is proper to remark that though whins are ſaid to be vitrified in a high degree of heat, yet this is not rigorouſly true, for in that

caſe they ſhould afford a tranſparent glaſs whoſe fracture would be perfectly poliſhed with a ſtrong luſtre, as we ſee that of common glaſs, whereas in truth they melt only into an *enamel* nearly approaching to the perfect vitreous ſtate; even the bottles made of them are nothing more; and hence their ſuperior hardneſs. Their ingredients therefore are not uniformly diffuſed through their whole maſs, but lie in the ſame order and poſition as before fuſion, and in effect they contain much more ſilex than can be completely vitrified by the ſmall proportion of lime and argil that enter into their compoſition, even though aſſiſted by the ſoda; and in the next place we muſt notice that the affinity of ſoda to ſilex diminiſhes in the ſame ratio as the heat diminiſhes and conſequently they ſeparate, if the heat be not ſo ſuddenly diminiſhed as to impede all motion. This is evident by what happens to common glaſs when ſlowly cooled down to the temperature of the atmoſphere, as came to paſs in the glaſs houſe at Leith, and conformably to this inſtance the ſaxification might take place even after a perfect vitrifaction. That there are unions grounded on chymical affinity which take place to a certain degree only at certain temperatures and are in great meaſure looſened at a lower temperature, appears in the common inſtance of the ſolution of moſt ſalts in water, ſpirit of wine or other menſtruum greater in a high degree of heat than at a lower, and has alſo been noticed

in

in the fuſion of gold in a maſs of ſilver, for if the ſilver be very gradually cooled the gold will ſeparate from it, as Homberg obſerved, Mem. Par. 1713.

Now the affinity of ſilex to the alkali, being looſened by a ſlight diminution of heat, the affinity of argil to the ſilex to which it united only as to a *compound* in the given temperature, is alſo neceſſarily looſened. That in the dry way argil unites to ſilex in temperatures below 150°, only in conſequence of the previous union of the ſilex to the alkali, is clearly deduced from this fact, that if the alkali be abſent the union will not take place in temperatures below 150°, whereas it takes place by Sir James's own experiment at temperatures below 100° when the alkali is preſent, for he found the whins fuſible at 55°. It is true the whins contain lime alſo, but though the preſence of a certain proportion of lime contributes materially to the fuſibility of ſilex and argil, yet it would be ineffectual in degrees of heat below 120° if an alkali were not preſent to aſſiſt it, as I know by experience.

The preſence of argil contributes alſo to the diminution of the affinity of the alkali to the ſiliceous ingredient, as the alkali ſeems to have nearly as ſtrong an affinity (ſome think ſtronger) to argil as to ſilex; hence it is that all Analiſts ſince Bergman's time employ an alkali to looſen the intimate union of ſilex and argil in precious ſtones.

Theſe

Theſe facts being duly conſidered, we ſhall not be ſurpriſed at ſeeing the cloſe vitreous texture deſtroyed by the ſlow cooling of melted whins (all of which contain the above ingredients) and ſucceeded by the looſer texture of a mere ſtony ſubſtance. This is the only change that takes place, if we except the minute and indeterminate cryſtallizations that occur in the cavities formed by the expulſion of air while the maſs was as yet ſoft, for the facettes interſperſed through the ſtone cannot be accounted cryſtals, but only the rudiments of cryſtallization. Theſe are formed at the inſtant the affinity of the alkali is leſſened and the earths begin to aſſume their ſolid ſtate. The alkali being as yet liquid, allows the earthy particles to move through it, and to form theſe incipient cryſtallizations.

We are now to examine how far the ſtony ſtructure aſſumed through ſlow refrigeration, by ſtones previouſly fuſed, tends to afford any ſupport to the Huttonian theory. In my opinion it affords none at all; the utmoſt effect it can produce in an unprejudiced mind is to render the origin of whins ambiguous by making them aſſume the appearance of a Neptunian origin, when in fact they owe it to fuſion; but it is only an *appearance*, for natural whins are accompanied with circumſtances and contain ſubſtances which contradict that appearance, and prove it to be deceitful. Beſides, theſe experiments have no relation whatſoever to granite or calcareous maſſes which form the bulk of the

the globe, and afford not the ſlighteſt indication of their origin; whins, though they abound in Scotland and ſome other countries, are in compariſon of the former but thinly ſcattered over the ſurface of the globe. Some reſemblance betwixt them and lavas has been long noticed. I ſhall now briefly mention a few of the diſcriminating characters of the artificial and natural whins, which may in moſt caſes prevent us from confounding them, or aſcribing to them a common origin.

1° The *natural* whins, particularly amygdaloids (vulgarly called toadſtones) frequently contain calcareous ſpar and zeolyte; now as the former contains fixed air, and the latter a notable proportion of water, I hardly think Sir James, who profeſſes not to agree with Dr. Hutton in all points, will allow theſe to have been vitrified or fuſed.

2° The *natural* whins, according to Dr. Kennedy's ſtatement, loſe five per cent. of water and other volatile matter when heated to redneſs. It is not ſaid whether the artificial loſe any part of their weight by ſuch treatment, but it is plain they would not, ſince even the lavas of Catania and Piedemonte, though of ancient date, loſt none, as Dr. Kennedy expreſsly notices, and has thus afforded an excellent criterion for diſtinguiſhing the long conteſted origination of theſe ſubſtances.

3° As

3° As Sir James has neglected giving a *complete* account of the external characters of the natural whins, which were the ſubject of his experiments, as alſo of the regenerated or artificial whins derived from them, and as I have not myſelf ſeen them, it is difficult for me to compare them with each other, and would indeed be impoſſible if ſome account of them had not been given by Mr. Pictet in his valuable Journal Britannique, copied into the 5th Vol. of the new Rozier's Journal, p 313. It is the reſult of the examination both of the natural and artificial whins by the Society of Natural Hiſtory at Geneva.

As to the natural grunſtein, No. 1. they remark that it betrays not the leaſt mark of an igneous origin, but that the whins which Sir James produced from it had every diſtinctive character of a *lava*, and even of a *porous lava*.

The baſalt (or rather trapp) on which the caſtle of Edinburgh ſtands is of a compact ſtructure; the artificial produced from it, Sir James tells us, ſo greatly reſembles it both in colour and texture that it would be difficult or perhaps impoſſible to diſtinguiſh them, *but for a few minute air bubbles, diſtinguiſhable in the artificial.* Neptuniſts will however conſider this as a leading character of diſtinction. The mineralogiſts of Geneva add, that the colour of the artificial is deeper, and its hardneſs greater than that of the natural. If the ſpecific gravity and other characters

characters of both were given, it is probable that other differences might be perceived. It is only in these characters that any difference can be expected, as the internal composition must be the same in both.

Of the remaining artificial whins I can give no account, their external characters having been omitted; I cannot however pass over the general inferences that Sir James deduces from his experiments, namely, "that the arguments against the subterraneous "fusion of whinstone, derived from its stony character, seem "now to be fully refuted," for not to repeat what has been already said, that many of them contain substances whose existence is incompatible with that hypothesis, I must farther add that the upright state in which many of them exist, for instance, the basaltic pillars of Staffa, and of the Giants Causeway, and of many other countries, the basis they rest on, sometimes granite, sometimes gneiss, sometimes coal or limestone, and the total absence of all signs of the operation of fire, forbid us to entertain any doubt of their production in the moist way. Nay the college of Dublin now possesses fragments of basaltic pillars in which marine shells are imbedded; if such evidence can be resisted it is in vain to seek for greater.

Sir James thinks the cause of the fluidity of lavas, which I formerly suggested, as strange and inconceivable as that of Citizen

Dolomieu. Not having had the happineſs of viewing thoſe ſtupendous torrents, I founded my opinion on the accounts given by the moſt accurate obſervers, and particularly of Citizen Dolomieu who beheld and carefully examined every circumſtance relating to them for many years. This great obſerver has not thought my opinion ſo inconceivable, for he has ſince embraced it; "from the "manner, ſays he, in which lavas flow, it cannot be doubted "but they carry with them a ſubſtance capable of maintaining "their heat and fluidity, and contain a ſubſtance which burns in "contact with the atmoſphere until it is conſumed. This ſub-"ſtance, of which ſulphur is at leaſt one of the principal ingre-"dients, if it be not the only one, bears a ſtrong reſemblance in "its conſtitution to phoſphorus, being capable of two ſorts of "combuſtion. This combuſtion ſeems capable of maintaining "fluidity in a bed of lava, &c." 1 New Rozier's Journ. p. 119 and 120.

Sir James ſays I have ſuppoſed ſubſtances that have left no trace of their exiſtence. Other obſervers however diſcovered theſe traces, as Dolomieu and Fabroni in the paſſages I have already quoted. Mineralogy, vol. 1. p. 397, and 1 New Roz. p. 120, 121. It is not to be expected that volatile ſubſtances, ſuch as ſulphur and petrol, ſhould long remain.

However,

However, I acknowledge that the cauſe of the ſtony appearance which lavas after cooling exhibit, diſcovered by Sir James, appears to me at preſent by far the moſt probable; and that in this reſpect his diſcovery is of great importance to geology. But I perſiſt in thinking his experiments afford no confirmation of the high degrees of heat attributed to volcanos, and ſtill leſs to the many hypotheſes gratuitouſly heaped on each other by Doctor Hutton, or to the volcanic origin of whins or traps, for the reaſons already aſſigned.

AN ILLUSTRATION *and* CONFIRMATION *of* SOME FACTS *mentioned in an* ESSAY *on the* PRIMITIVE STATE *of the* GLOBE.* *By* RICHARD KIRWAN, *Esq*; *L. L. D. F. R. S. and P. R. I. A.*

Read May 5th, 1800.

THERE is a remarkable fact, ſtated in this eſſay, with reſpect to which the Moſaic account is fully at iſſue with the moſt plauſible of the lately deviſed philoſophic theories of the primitive ſtate of the globe, namely, the emerſion of ſome portion of land from the primæval ocean, previous to the creation of fiſh. Moſes expreſsly affirming, Buffon, the anonymous author of *L'hiſtoire du Monde primitif*, and many others, expreſsly denying it. In proof of the Moſaic account I alleged that no petrifactions were found imbedded and incorporated in maſſes of ſtone in ſuch countries as were elevated 8500 or 9000 feet above the actual level of the ſea; for inſtance, in the great Tartarian platform and the elevated regions of Siberia, though in all inferior regions of the ſame extent ſuch petrifactions were abundantly found, at leaſt in limeſtones; but even in theſe none were found in thoſe elevated tracts, as I proved by the teſtimonies

* See Vol. VIth of the Tranſactions of the Academy.

nies of all the philoſophic travellers who have traverſed and examined them.

Bouguer and Don Ulloa * atteſt the ſame fact with reſpect to Quito and the lofty regions of South America, having met with none in ninety leagues from a little north of the Æquator to Cuenca, between 2° and 3° ſouth of it, Quito being elevated 9374 feet above the level of the ſea. I even doubted whether the petrifactions found by Mr. De Luc on Mount Grenier at the height of 7800 feet, were incorporated in the body of any ſtony maſs, but he has ſince aſſured me that they were; now the ſhells he found, being *Cornua Ammonis*, a ſpecies of thoſe called *Pelagicæ*, it would thence appear that the ſea had riſen ſtill higher, though not above 100 feet.

To repel this proof of the Moſaic account, it has been replied by the laborious, learned and eloquent writer of the *L'hiſtoire du Monde primitif* and others, that the keen air exiſting in theſe elevated regions had long ago decompoſed and conſumed the ſhells that might have been there depoſited; but as the ſtones ſtill remain, it is evident that the ſhells incorporated in their interior muſt alſo have remained, if any ſuch were ever contained in them.

However,

* Bouguer Figure de la Terre, p. lxv. Don Ulloa, Mem. Philoſoph. vol. I. p. 363.

HOWEVER, it is inſiſted that petrified ſhells have been found at a far ſuperior height to that which I ſtated as the higheſt limit at which they could be found, namely 9000 feet, for that thoſe which I quoted as proofs of a ſubſequent deluge, found, according to Don Ulloa, at the height of 14,220 feet in Peru, were in reality petrified. To this account, as it was only a hearſay report of Gentil, who had it from Don Uiloa, I own I did not give full credit, at leaſt I thought the circumſtance of their having been petrified and imbodied in a rock not probable; ſince that time I have met with a work, compoſed by Don Ulloa himſelf, intitled *Memoires Philoſophiques, Hiſtoriques & Phyſiques*, in the firſt vol. of which, p. 34 and 364, the fact of the ſhells being petrified and imbodied in a rock is fully ſtated and confirmed; but I ſhall here ſatisfactorily prove that the height at which they were found did not exceed, nor even equal 9000 feet above the level of the ſea. And, ſecondly, that they were left there by a deluge that ſucceeded the emerſion of land from the primæval ocean.

1°. GENTIL tells us, Mem. Par. 1771, p. 439 in 8vo. that theſe ſhells were found on a mountain near Guancavelica, a ſmall town or village between 12° and 13° ſouth latitude, which mountain he ſays is far from being one of the higheſt of the Cordelieres, and that on the ſpot in which the ſhells were found the mercury in the barometer ſtood at the height of 17 inches, one line and $\frac{1}{4}$=17,103 inches, from which he infers that this ſpot was elevated 2200$\frac{1}{3}$ toiſes over the level of the ſea, = 13,869 Engliſh feet. This height I ſhall now ſhew to have been falſely eſtimated.

DON

Don Ulloa tells us, p. 34 of the above-mentioned work, and Gentil repeats after him, that, at the village of Guancavelica, near, but ſomewhat below this ſpot, the mercury ſtood at the height of 18 inches, one line and $\frac{1}{2}$. Now Bouguer, *Figure de la Terre*, p. xxxvi, expreſsly ſays that Quito is elevated from 14 to 1500 toiſes only over the level of the ſea, and is the higheſt *inhabited* part of the globe; Guancavelica, therefore, which is inhabited, and muſt have been well known to Bouguer, cannot be ſo high as Quito. Quito was geometrically meaſured, Guancavelica was not.

Bouguer alſo tells us, *ibid.* in note, that the mercury at Quito ſtands at the height of twenty inches and one line; how then is it poſſible that it ſhould ſtand at eighteen inches and one line at Guancavelica which muſt be much lower, if the barometer were not ill conſtructed? No difference of temperature between Quito and Guancavelica could cauſe ſuch an enormous difference as two inches in the mercurial height. The art and neceſſity of freeing barometers from air were not generally known * before Mr. De Luc's immortal work on the modifications of the atmoſphere, which was not

* I ſay not generally known, becauſe though Mr. Du Fay had ſhewn the advantage of expelling air from the mercury in the tube, in Mem. Par. 1723, and Caſſini had followed that method in meaſuring the heights of Puy de Dome and Mont D'Or in 1740, yet it was not generally adopted until Mr. De Luc had proved its neceſſity and perfected the method of performing it in 1772. See 1 De Luc, p. 58.

not publiſhed 'till upwards of thirty years after Don Ulloa's experiments. The air then remaining in his barometer, which he probably filled at Lima, in whoſe territory Guancavelica lies, under a preſſure of twenty-eight or between twenty-ſeven and twenty-eight (French) inches, muſt have conſiderably expanded and depreſſed the column of mercury under it, when brought up a mountain of perhaps 7000 or 8000 feet high; and hence this mercurial column remained ſo low at Guancavelica as eighteen inches and one line.

That in theſe circumſtances the air contained in the barometer might cauſe a difference of three inches or more between the height at which mercury purged of air would ſtand, appears by the experiments of Caſſini, Mem. Par. 1740, *Sur la Meridienne de Paris*, p. clxxii. for he found that the mercury, freed from air by ebullition in the tube, ſtood four or five lines higher than in barometers filled without that precaution. Nay, Cardinal Luynes found the difference betwixt ſuch barometers to amount ſometimes to fourteen lines, Mem. Par. 1768, p. 490 in 8vo. How great muſt it therefore be in barometers tranſported to greater heights than that at which they were filled?

Moreover, Don Ulloa expreſsly tells us that the mountain on which theſe ſhells were found was *every where habitable*, *Mem. Philoſophiques*, p. 34 and 35, which it could not be, and would be expreſsly contradicted by Bouguer, if its height were 13,000 Engliſh feet over the ſurface of the ſea. From all which I con-

clude that the height of the mercurial column, if the barometers had been properly conſtructed, would have been about 22 French inches =23, 44 Engliſh, and as the temperature was remarkably cold this would indicate a height of about 8200 feet above the level of the ſea.

But, ſecondly, let the height be what it may, it is certain that theſe ſhells were depoſited there after the emerſion of land from the primitive ocean, and conſequently by a ſubſequent deluge; for Don Ulloa expreſsly tells us, that in the ſame rocks in which theſe ſhells are found, *petrified wood* is alſo found, Mem. Philoſophiques, p. 372. This wood muſt have grown on dry land, and muſt have been floated when the ſhells were depoſited, ſince both are found in the ſame rocks. It muſt have been brought thither by a deluge, as no wood can at preſent grow there, as Don Ulloa alſo atteſts. The ſhells are for the moſt part bivalves, which geologiſts allow to form petrifactions of the moſt modern date.

Lastly, La Peyrouſe mentions that he diſcovered marine ſhells on Mount Perdu, the higheſt of the Pyrenees, at the height of nearly 10,000 feet. How he aſcertained the height is not mentioned; but that theſe ſhells were depoſited by the deluge is certain, for at the ſame height he found alſo the bones of land animals petrified, 37 Journ. des Mines, p. 59, 60 and 64.

AN ESSAY *on the* DECLIVITIES OF MOUNTAINS.

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AMONG the various causes to whose activity the planet we inhabit owes its present wonderfully diversified appearance, some undoubtedly exerted their influence from its very origin, and others at subsequent periods; of these last one at least, namely, the Noachian deluge, was universal in its operation, while the effects of many more were partial and local, such as those resulting from earthquakes, volcanos, particular inundations, &c.

IN a general survey of the globe it is only to general causes whose operation was universal that our attention can be directed, the effects of partial causes being the proper objects of the geological history of those countries that were particularly affected by them.

But to diſtinguiſh cauſes of the former claſs from thoſe whoſe operation was more confined, it is neceſſary to diſcover ſome character by which their effects may unequivocally be diſcerned.

Now a general *uniformity*, or agreement in ſome particular circumſtance in every part of the globe, ſeems to be a ſure teſt of the operation of ſome general cauſe. The diſcovery of uniform appearances is therefore of primary importance in geological reſearches. In the preſent eſſay I ſhall confine myſelf to the inveſtigation of one inſtance of this ſort, namely, the *inequality of declivity* which the ſides or flanks of mountains exhibit in every part of the globe hitherto examined according to the points of the compaſs to which they face, and are expoſed.

That one part of almoſt every high mountain or hill is ſteeper than another could not have eſcaped the notice of any perſon who had traverſed ſuch mountains, but that nature in the formation of ſuch declivities had any regard to different aſpects or points of the compaſs, ſeems to have been firſt remarked by the celebrated Swediſh Geologiſt Mr. Tilas in the 22d vol. of the Memoires of Stockholm for 1760.* Neither Varenius, Lulolph nor Buffon in his Natural Hiſtory publiſhed in 1748, have noticed this remarkable circumſtance.

The

* See alſo vol. 25, Swed. Abhandl. p. 291, where *Cronſted* explains ſome obſcure parts of Tilas's obſervation.

The obſervation of Tilas however relates only to the extreme ends, and not to the flanks of mountains; with reſpect to the former he remarked that the *ſteepeſt* declivity always faces that part of the country where the land lies loweſt, and the *gentleſt* that part of the country where the land lies higheſt, and that in the ſouthern and eaſtern parts of Sweden they conſequently face the E. and S E. and in the northern the W. The eſſential part of this obſervation extends therefore only to the general elevation or depreſſion of the country, and not to the bearings of theſe declivities.

The diſcovery that the different declivities of the flanks of mountains bear an invariable relation to their different aſpects ſeems to have been firſt publiſhed by Mr. Bergman in his phyſical deſcription of the earth, of which the ſecond edition appeared in 1773. He there remarked that in mountains that extend from N. to S. the weſtern flank is the *ſteepeſt* and the eaſtern the *gentleſt*. And that in mountains which run E. and W. the ſouthern declivity is the ſteepeſt and the northern the gentleſt, vol. 2d. § 187.

This aſſertion he grounds on the obſervations related in his firſt vol. § 32, namely, that 1° in Scandinavia the Suevoberg mountains that run N. and S. ſeparating Sweden from Norway, the weſtern or Norwegian ſides are the ſteepeſt, and the eaſtern

or

or Swediſh the moſt moderate, the verticality or ſteepneſs of the former being to that of the latter as 40 or 50 to 4 or 2.*

2dly. That the Alps are ſteeper on their weſtern and ſouthern ſides than on the eaſtern and northern.

3dly. That in America the Cordelieres are ſteeper on the weſtern ſide, which faces the Pacific Ocean, than on the eaſtern. But he does not notice a few exceptions to this rule in particular caſes which will hereafter be mentioned.

Buffon, in the firſt vol. of his Epochs of Nature publiſhed in 1778, p. 185, is the next who notices the general prevalence of this phænomenon, as far as relates to the eaſtern and weſtern ſides of the mountains that extend from north to ſouth, but he is ſilent with reſpect to the north and ſouth ſides of the mountains that run from eaſt to weſt; nay, he does not ſeem to have had a juſt comprehenſion of this phænomenon, for he conſiders it conjointly with the general dip of the regions in which theſe mountains exiſt. Thus he tells us, vol. 1ſt. p. 185, that in all continents the general declivity, taking it from the ſummit of mountains, is always more rapid on the weſtern than on the eaſtern ſide, thus the ſummit of the chain of the Cordelieres is much nearer to the weſtern ſhores than to the eaſtern; the chain which divides the whole length of Africa from the Cape of Good Hope

* The verticality of the ſides is inverſely as the length of the deſcent.

Hope to the mountains of the Moon is nearer, he ſays, to the weſtern than to the eaſtern ſeas; of this however he muſt have been ignorant, as that tract of country is ſtill unknown.

The mountains which run from Cape Comorin through the peninſula of India are, he ſays, much nearer to the ſea on the eaſt than on the weſt; he probably meant the contrary, as the fact is evidently ſo, and ſo he ſtates it in the 2d vol. p. 295; the ſame he tells us may be obſerved in iſlands and peninſulas, and in mountains,

This remarkable circumſtance of mountains was notwithſtanding ſo little noticed that in 1792 the author of an excellent account of the territory of Carlſbad in Bohemia tells us he had made an obſervation, which he had never met with in any phyſical deſcription of the earth, namely, that the ſouthern declivity of all mountains was much ſteeper than the northern, which he proves by inſtancing the *Erzgebirge* of Saxony, the Pyrenees, the mountains of Switzerland, Savoy, Carinthia, Tyrole, Moravia, the Carpathian and Mount Hæmus in Turkey, 2, Bergm. Jour. 1792, p. 385, in the note.

Herman in his Geology, publiſhed in 1797, p. 90, has at leaſt partially mentioned this circumſtance, for he ſays that the eaſtern declivities of all mountains are much gentler and more thickly covered with ſecondary ſtrata, and to a greater height, than the weſtern

weſtern flanks, which he inſtances in the Swediſh and Norwegian mountains, the Alps, the Caucaſian, the Appenine and Ouralian mountains; but the declivities bearing a ſouthern or northern aſpect he does not mention.

La Metherie, in the 4th vol. of his Theory of the Earth, of which the ſecond edition appeared in 1797, a work which abounds in excellent obſervations, p. 38:, * produces numerous inſtances of the inequality of the eaſtern and weſtern declivities, but ſcarce any of the northern and ſouthern, whoſe difference he does not ſeem to have noticed, but he makes a remark which I have not ſeen elſewhere, that the coaſts of different countries preſent ſimilar declivities.

With regard to eaſtern and weſtern aſpects he thinks that a different law has obtained in Africa from that which has been obſerved in other countries, for in that vaſt peninſula he imagines the eaſtern declivities of mountains are the ſteepeſt and the weſtern the gentleſt. Of this however he adduces no other proof but that the greateſt rivers are found on the weſtern ſide; this proof ſeems inſufficient, as, if mountains be ſituated far inland, great rivers may flow indiſcriminately from any ſide of them, and ſometimes few rivers flow even from the ſide whoſe deſcent is moſt moderate, for inſtance, from the eaſtern ſide of the mountains of Syria; the Elbe and the Oder, two of greateſt rivers in Germany,

* It is to be regretted that he ſcarce ever quotes his authorities.

Germany take their courſe from the weſtern ſides, the firſt of the Bohemian and the other of the Moravian mountains which yet are the ſteepeſt; many originate from lakes, as the Shannon with us; many take ſuch a winding courſe that from a bare knowledge of the place of their diſemboguement it is impoſſible to judge from what ſide of a mountain they iſſue, if from any; their courſe at moſt diſcovers the depreſſion of the general level of the country.

In 1798, the celebrated traveller and circumnavigator, John Reinhold Foſter, publiſhed a geological tract which merits ſo much more attention as all the facts were either obſerved by himſelf or related to him by the immediate obſervers. In this he ſtates as a fact univerſally obſerved, that the ſouth and ſouth eaſt ſides of almoſt every mountain are ſteep, but that the north and north weſt ſides are gently covered and connected with ſecondary ſtrata in which organic remains abound, which he illuſtrates by various inſtances, ſome of which have been already, and others will preſently be mentioned.

At preſent this fact attracts the greateſt attention being obviouſly connected with the original ſtructure of the globe and clearly proving that mountains are not mere fortuitous eruptions unconnected with tranſactions on the ſurface of the earth as has of late been confidently advanced.

I SHALL now ſtate the principal obſervations relative to this object that have been made in different parts of the world.

In Europe.

1° THE mountains that ſeparate Sweden from Norway extend from north to ſouth, their weſtern ſides are *ſteep* and the eaſtern *gentle*, 1. Bergm. Erde Beſchreib. p. 157.

2° THE Carpathian mountains run from E. to W. their ſouthern ſides towards Hungary are ſteep, their northern towards Poland moderate, Foſter, § 46.

3° Doctor Walker, profeſſor of natural hiſtory at Edinburgh, obſerved that the coaſts and hills of Scotland are ſteeper and higher on the weſtern ſide than on the eaſtern Jamiſon Mineralogy of Shetland. p. 3, However Jamiſon obſerved that the ſouth ſide of the iſle of Aran is the loweſt and the north ſide the higheſt, p. 51.

4° THE mountains of Wales are gentle on the eaſtern and ſteep on the weſtern ſides.

5° THE mountains of Parthery in the county of Mayo are ſteep on the weſtern ſide.

6° THE mountains which ſeparate Saxony from Bohemia deſcend gently on the Saxon or northern ſide, but are ſteep on the

Bohemian or ſouthern ſide Charpent, p. 75. The ſouthern declivity is to the northern as ſix to two, 2d Bergm. Journ. 1792, p. 384 and 385.

7° The mountains which ſeparate Sileſia from Bohemia run nearly from E. to W. yet are ſteeper on the northern or Sileſian ſide than on the oppoſite Bohemian, Aſſemanni *Sileſia*, 335. Such branches as run from N. E. to S. W. have their weſtern covered with primordial ſtrata and conſequently leſs ſteep, 4, New Roz. p. 157.

8° The Meiſſener in Heſſia is ſteeper on the N. and E. ſides which face the Warra than on the ſouth and weſtern, 1, Bergm. Journ. 1789, p. 272.

9° The mountains of the Hartz and Habichtswald are ſteep on the ſouth and gentle on the northern ſides, Foſter § 46.

10° The Pyrenees which run from E. to W. are ſteeper on the ſouthern or Spaniſh ſide, Carbonieres XIII.

11° The mountains of Crim Tartary are gentle on the northern and ſteep on the ſouthern ſides, Foſter, ibid.

In Aſia.

12° The Ourals which ſtretch from N. to S. are far ſteeper on weſtern than on the ſouthern ſides, Herman Geologie, p. 90, and 2d Ural Beſchreib. p. 389.

13° The mountain of Armenic to the weſt of the Ourals is ſteep on its E. and N. ſides, but gentle on the ſouthern and weſtern, 1. Pallas Voy. p. 277.

14° The Altaiſchan mountains are ſteep on their ſouthern and weſtern ſides, but gentle on the northern and eaſtern, Foſter, ibid. and Herman 2d Ural Beſchreib. p 390 in the note.

15° So alſo are the mountains of Caucaſus, 3d Schrift. Berl. Geſelsch. 471.

16° The mountains of Kamſkatſka are ſteep on the eaſtern ſides, Pallas, 1 Act. Petropol. 1777, p. 43.

17° The Ghauts in the Indian peninſula are ſteep on the weſtern ſides.

18° The mountains of Syria which run from N. to S. ſkirting the Mediterranean, are ſaid to be ſteeper on the weſtern ſide facing the Mediterranean, 4. La Metherie, p. 380.

In America.

The Cordelieres run from N. to S. their weſtern flanks towards the Pacific are ſteep, their eaſtern deſcend gradually.

In Guiana there is a chain of mountains that run from E. to W. their ſouthern flanks are ſteep, their northern gentle, Voyages de Condamine, p. 140.

To

To aſſign the cauſes of this almoſt univerſal alotment of unequal declivities to oppoſite points and why the greateſt are directed to the weſt and ſouth, and the gentleſt on the contrary to the eaſt and north it is neceſſary to conſider,

1° That all mountains were formed while covered with water.

2° That the earth was univerſally covered with water at two different æras, that of the creation, and that of the Noachian deluge.

3° That in the firſt æra we muſt diſtinguiſh two different periods, that which preceded the appearance of dry land, and that which ſucceeded the creation of fiſh, but before the ſea had been reduced nearly to its preſent level; during the former, the primæval mountains were formed, and during the laſt, moſt of the ſecondary mountains and ſtrata were formed.

4° That all mountains extend either from E. to W. or from N. to S. or in ſome intermediate direction between theſe cardinal points which need not be particularly mentioned here, as the ſame ſpecies of reaſoning muſt be applied to them, as to thoſe to whoſe aſpect they approach moſt.

These preliminary circumſtances being noticed, we are next to obſerve that during the firſt æra, this vaſt maſs of water moved in

in two general directions, at right angles with each other, the one from E. to W. which needs not to be proved, being the courſe of tides which ſtill continue, but were in that ocean neceſſarily ſtronger and higher than at preſent: the other from N. to S. the water tending to thoſe vaſt abyſſes then formed in the vicinity of the ſouth pole, as ſhewn in my former eſſays. Before either motion could be propagated a conſiderable time muſt have elapſed.

Now the primæval mountains formed at the commencement of the firſt æra, and before this double direction of the waters took place, muſt have oppoſed a conſiderable obſtacle to the motion of that fluid in the ſenſe that croſſed that of the direction of theſe mountains. Thus the mountains that ſtretch from N. to S. muſt have oppoſed the motion of the waters from E. to W. this oppoſition diminiſhing the motion of that fluid diſpoſed it to ſuffer the earthy particles with which in thoſe early periods it muſt have been impregnated to chryſtallize or be depoſited on theſe eaſtern flanks, and particularly on thoſe of the higheſt mountains, for over the lower it could eaſily paſs; theſe depoſitions being inceſſantly repeated at heights gradually diminiſhing as the level of the waters gradually lowered, muſt have rendered the eaſtern declivities or deſcent, gentle, gradual and moderate, while the weſtern ſides receiving no ſuch acceſſions from depoſitions muſt have remained ſteep and craggy.

AGAIN,

Again, the primeval mountains that run from E. to W. by oppoſing a ſimilar reſiſtance to the courſe of the waters from N. to S. muſt have occaſioned ſimilar depoſitions on the northern ſides of theſe mountains againſt which theſe waters impinged, and thus ſmoothed them.

Where mountains interſect each other in an oblique direction, the N E ſide of one range being contiguous to the S W. flanks of another range, there the afflux of adventitious particles on the north eaſt ſide of the one, muſt have frequently extended to the S. W. ſide of the other, particularly if that afflux were ſtrong and copious; thus the *Erzgebirge* of Saxony, which run from W. to E. have their N E. ſides contiguous to the S W. ſide of the *Rieſengebirge* that ſeparate Sileſia from Bohemia, and hence theſe latter are covered with the ſame beds of Gneiſs, &c. as the northern ſides of the Saxon, and thereby are rendered ſmooth and gentle comparatively to the oppoſite ſide, which being ſheltered, remains ſteep and abrupt, which explains the ſeventh obſervation.

The cauſes here aſſigned explain why the covering of adventitious ſtrata on the higheſt mountains is generally thinneſt at the greateſt height, and thickeſt towards the foot of the mountain, for the bulk of the water that contained the adventitious particles being proportioned to its depth, and the maſs of earthy particles

particles with which it was charged, being proportioned to the bulk of water that contained them, it is plain, that as the height of water gradually decreaſed, the depoſitions from it on the higher parts of the mountains muſt have been leſs copious than on the lower, where they muſt have been oftener repeated.

Hence 2° granitic mountains, generally the moſt antient, frequently have their northern or eaſtern ſides covered with ſtrata of gneiſs or micaceous ſhiſtus, and this often with argillite, or primæval ſand-ſtone, or lime ſtone, theſe being either of ſomewhat later formation or longer ſuſpendible in water.

Hence 3° different ſpecies of ſtone are often found at different heights of the ſame flank of a mountain, according as the water which conveyed theſe ſpecies, happened to be differently impregnated at different heights; during the firſt æra its depoſitions formed the primitive ſtony maſſes, but after the creation of fiſh, lime ſtone, ſand-ſtone, farcilites and ſecondary argillites in which piſcine remains are found, were depoſited. But during the ſecond æra, viz that of the Noachian deluge, by reaſon of the violence and irregularity of its aggreſſion, the depoſitions were more miſcellaneous and are found at the greateſt heights; yet in general they may well be diſtinguiſhed by the remains of land animals, or of vegetables, or of both, which they preſent in their ſtrata (or at leaſt by the impreſſions of vegetables which they bear) as theſe muſt have been conveyed after the earth had been,

been inhabited. But mountains regularly ſtratified bearing ſuch remains, for inſtance the Carboniferous, cannot be deemed to have been formed in a period ſo tumultuous. During this deluge the waters alſo held a different courſe, proceeding at firſt from ſouth to north and afterwards in both oppoſite directions in our weſtern hemiſphere as ſhewn in treating of that cataſtrophe in my ſecond eſſay.

Hence, and from various contingent local cauſes, as partial inundations, earthquakes, volcanos, the eroſion of rivers, the elapſion of ſtrata, diſintigration, the diſruption of the lofty mounds by which many lakes were antiently hemmed in, ſeveral changes were produced in particular countries that may at firſt ſight appear, though in reality they are not, exceptions to the operation of the general cauſes already ſtated.

Thus the mountains of Kamſkatſka had their eaſtern flanks torn and rendered abrupt by the irruption of the general deluge probably accompanied by earthquakes. And thus the Meiſſener had its E. and N. flanks undermined by the River Warra, as Werner has ſhewn; thus the eighth and ſixteenth obſervations are accounted for; as is the thirteenth by the vaſt inundations ſo frequent in this country, 1. Pallas, p. 172, which undermined or corroded its E. ſide while the weſtern were ſmoothed by the calcareous depoſitions from the numerous rivers in its vicinity.

Hence 4° We ſee why on different ſides of lofty mountains different ſpecies of ſtones are found, as Pallas and Sauſſure have obſerved, 2. Sauſſ. § 981, a circumſtance which Sauſſure imagined almoſt inexplicable, but which Dolomieu has ſince happily explained by ſhewing that the current which conveyed the calcareous ſubſtances to the northern, eaſtern and N. eaſtern ſides of the Alps, for inſtance, was ſtopped by the height of theſe mountains, and thus prevented from conveying them to the ſouthern ſides, and thus the N. eaſtern ſides were rendered more gentle than the oppoſite, 3, New Roz. p. 425, conformably to the theory here given.

Hence 5° Where ſeveral lofty ridges run parallel to each other it muſt frequently happen that the external ſhould intertercept the depoſitions that do not ſurmount them, and thus leave the internal ridges ſteep on both ſides.

Hence 6° Low granitic or other primitive hills are frequently uncovered by adventitious ſtrata on all ſides as at Phanet in the county of Donegal, or are covered on all ſides; the impregnated waters either eaſily paſſing over them or ſtagnating upon them according to the greater or leſſer rapidity of its courſe and the obſtacles it met with.

The two fold motion of the antient ocean is noticed both by Buffon and Bergman, but neither of them have deduced from it

it the true explanation of the phænomena of which we here treat; Buffon attributes the formation of ſecondary mountains to depoſition or ſediments from the ſea after the exiſtence of fiſh, 1, Epoques, p. 143, in 8vo. which he ſays inveſted the baſes of mountains without noticing any diſtinction of ſides, p. 144 and 170. He thinks theſe ſediments were equally conveyed from both poles towards the Æquator, for it is the Æquatorial regions that he thinks thoſe mighty caverns opened towards which the primitive ocean was impetuouſly borne and in which it was ingulphed, p. 181, 182 and 183. If ſo, ſimilar declivities ſhould be formed on the ſouthern as on the northern ſides of mountains, which is contrary to the obſerved facts. His explanation of the eaſtern and weſtern declivities is defective and erroneous, for he attibutes the abruptneſs of the weſtern ſides to the eroſion of the coaſts on that ſide (an eroſion that exiſts only in fancy) and the ſmoothneſs of the eaſtern to the gradual deſertion and retreat of the ſea on that ſide, p. 184 and 185, a retreat equally fictitious as De Luc has well ſhewn. Whereas ſince the general motion of the ſea is from E. to W. if the eroſion were of either ſide it ſhould rather be on the eaſtern than on the weſtern; beſides, if the gentle declivities of the eaſtern ſides of mountains aroſe from the gradual retreat of the ſea the petrifactions of the ſecondary mountains thus formed ſhould conſiſt of ſuch ſhell fiſh as inhabit ſhallow

ſeas or ſhores, whereas they conſiſt chiefly of thoſe called *pelagicæ* which inhabit the greateſt depths.*

With reſpect to the eaſtern and weſtern declivities, Mr. Bergman's account of the origin of their inequality agrees exactly with mine, 2. Bergm. Erdeklotet § 183 and 187, but he fails in accounting for the inequality of the northern and ſouthern, for he ſuppoſes the courſe of the waters to tend equally from both poles towards the Æquator which would render the depoſitions equal on both ſides, which is contrary to obſervation.

* 2. Bergm. Erdekugel, p. 315.

OF CHYMICAL *and* MINERALOGICAL NOMENCLATURE. *By* RICHARD KIRWAN, *Esq; L. L. D. F. R. S. and P. R. I. A.*

Read March 24th 1800.

THE names given to the different ſubſtances known in common life, whether occurring in nature or produced by art, are coeval with languages themſelves, and whether all were merely conventional or ſome of them grounded on ſome relation to the thing ſignified is now of little importance to inquire, as from long habit both are equally immediately referred to the thing ſignified, without any reflexion on the original reaſon of their impoſition. Thus though the name *barometer* was originally impoſed on the inſtrument ſo called, becauſe the terms that compoſe this name *Baros* and *Metron* denote its uſe in meaſuring the weight of the atmoſphere, yet the name is juſt as well underſtood by thoſe who are totally unacquainted with its etymology as by thoſe to whom this is perfectly known. The inſtrument itſelf and not its uſe is denoted by the name, and equally occurs

to

to both; the workman who knows not its uſe knows what the name immediately ſignifies as well as the philoſopher who employs it, and as well as the German who calls it a *ſchwermeſſer*, whoſe primitive components alſo expreſs its uſe, nor is it better underſtood by either than the name *ſcrew* or *ſchraube*, which cannot be reſolved into any primitive component terms. Who on hearing the Latin name of a book ever thinks of its primitive ſignification—the bark of a tree? Languages muſt have been invented long before either chymiſtry or mineralogy were in any degree cultivated. In both the analytic and ſynthetic branches of chymiſtry as well as mineralogy, many ſubſtances muſt have occurred to which in common language no name was applied, yet the neceſſity of denoting them by ſome name was urgent. Here then a difficulty occurred, which ſome ſought to ſurmount by a name *arbitrarily* impoſed, others by names derived from ſome real or fanciful relation of the given ſubſtance to ſome uſe, object, perſon, or particular quality or circumſtance. Thus in chymiſtry *alcohol* appears to be a name arbitrarily impoſed on highly rectified ſpirit of wine or very ſubtle powder.* *Mercurius vitæ* denotes an antimonial preparation of great efficacy; as *Kermes mineral* does one that reſembles that ſubſtance in colour; *Glauber's ſalt*, a particular ſubſtance firſt formed by Glauber; *Epſom*, a ſalt firſt diſcovered in the ſprings near that town, &c. So in mineralogy, *quartz* ſeems to be a name arbitrarily impoſed, and *ſpath*, a name originating from the reſemblance of the integrant

* Some derive it from the Arabic *Kabala* exaruit.

grant *lamellæ* to a blade, and *ſelenite* from ſome fictitious reſemblance to the moon. The Etimology of theſe names was however ſoon forgotten or not attended to, and conſequently ſuch of them as conſiſted of only one word, not evidently denoting ſome falſe relation, might without inconvenience be admitted; but in proceſs of time deſcriptions were admitted inſtead of names, and theſe often falſe or abſurd, as *ſal mirabile*, *ſal ſecretum*, *arcanum duplicatum*, *arcanum corrallinum*, &c. The inconvenience and evident impropriety of many of theſe deſignations at laſt attracted the attention of thoſe eminent philoſophic chemiſts, Bergman and Morveau. Morveau ſo early as the year 1782 publiſhed an excellent Memoir on this ſubject in Rozier's Journal, and his ſentiments met with the entire approbation of Bergman. The new denominations he ſought to introduce ſoon however gave way to the ſtill more exquiſitely deviſed ſyſtematic appellations grounded on the diſcoveries of Lavoiſier, and the total elimination of the phlogiſtic element formerly admitted. Theſe, in the formation of which Morveau alſo, in concurrence with a few of the moſt eminent Pariſian chymiſts, bore a conſiderable part, were ſince admitted and recognized by moſt European chymiſts, and particularly in England. The exceptions that appeared to me reaſonable to the general rules laid down by this highly reſpectable aſſociation, or to ſome of the terms they introduced, I thought of too little conſequence to mention, knowing that the few antient denominations I retained, and the ſtill fewer new ones I introduced,

introduced, were perfectly intelligible, nor ſhould I attempt at preſent to vindicate them had I not perceived they attracted the cenſure of many on whoſe eſteem I ſet the higheſt value. Thus circumſtanced I feel myſelf juſtified in examining the general propriety of thoſe rules and aſſumed principles from which I thought proper to deviate, and of the denominations which I reject.

The firſt principle laid down by M. Morveau is, " that phraſes, " are not a name, that ſubſtances and chymical products ſhould " be denoted by names fit to indicate them on every occaſion, " without having recourſe to circumſtances, p. 373;" to this principle I give my intire aſſent.

Another rule laid down by M. Morveau is, " that in chuſing " denominations we ſhould prefer thoſe which have their roots " in the dead languages more generally known, in order that " the ſenſe ſhould ſuggeſt the name, and the name the ſenſe." It is for this reaſon combined with the firſt that I prefer ſingle names already underſtood and well known by all thoſe that have attained any knowledge of chymiſtry to new names derived from the Greek or new unknown barbarous Latin names. Hence I prefer the term *hepar*, denoting in all chymical authors a combination of ſulphur to every baſis except a metallic, to the barbarous unknown Latin term *ſulphuret;* and with reſpect to metals I prefer

prefer the well known terms *pyrites* and *galena* to the newly deviſed *ſulphuret of iron*, or *copper*, or *lead*; the former has the advantage of denoting the particular ſpecific combinations of ſulphur with iron, copper, arſenic or cobalt, and of excluding not only other ſulphurated metals that do not give fire with ſteel, as the vitreous ſilver ore, ſulphurated antimony, or lead, cinnabar, blende, ſulphurated biſmuth, &c. but alſo ſuch compounds even of copper and ſulphur as do not give fire with ſteel, as the vitreous copper ore and the grey copper ore and the purple. On theſe and many other conſiderations which will preſently be mentioned, I hope the ingenious M. Muſhet will recal his wiſh that I had modelled my nomenclature on the principles of the French School, 2. Phil. Magaz. p. 156. C. Faujas though of that ſchool remarks that theſe uſual names (when otherwiſe faultleſs) cannot without great inconvenience * be changed for others either erudite or barbarous, 35. Journ. des Mines, 894. Nor is his opinion in the

* This puts me in mind of ſome Latin puriſts who change known modern names into what they eſteem purer Latin ones. Thus inſtead of *cancellarius* they ſay *præfectus juris*, and for which Lipſius Not. ad Lib. I. Politicorum Cap. 9° juſtly cenſures the hiſtorian Paulus Æmilius. " Vetuſtatis etiam neſcio quid affectat in " nominibus hominum, locorum, urbium immutandis et in veterem formam redi- " gendis, ſæpe erudite, interdum vane, ſed ut ego judico, ſemper indecore, quorſum " *chartierius*, Gallorum cancellarius, *quadrigarius* mihi fit? et ille ipſe cancellarius " appellatur *præfectus juris?* et ubique Rex *Tarraconenſis*, qui nobis et majoribus " fuit Arragoniæ? talia infinita ſunt *audacter* et *ambitioſe* innovata et cum fraude " pariter ac *cruce* lectoris. The Greek and Roman hiſtorians were frequently guilty of the ſame fault which occaſions at this day much perplexity.

leaſt invalidated by the note of C. Coquebert, who aſſerts that the new are more exact, ſonorous and ſignificative; for any term is exact and ſignificative when it denotes without obſcurity or ambiguity the thing ſignified; and as to *ſound* it may deſerve attention in poetry, but ſurely, if not exceeding uncouth, it deſerves none in ſcience. He acknowledges that theſe alterations may cauſe ſome embarraſſment to the preſent race of men, but thinks that ſucceeding generations will bleſs thoſe that introduced them; I think on the contrary that they will curſe them for obliging them to learn both the new and the old denominations under the penalty of not underſtanding Stahl, Henckel, Margraf, Lemery, Geoffroy, Duhamel, Macquer, Bergman, Scheele and many others of the higheſt merit; can any one be ſo arrogant as to pretend that theſe immortal authors can become unintelligible without prejudice to the ſcience? is it poſſible that muſicians ſhould judge more ſhrewdly than thoſe who profeſs to be philoſophers? yet theſe have rejected all the modern alterations of notation that have been propoſed to them, though attended with ſome advantages, from the ſingle conſideration that if new modes were adopted the inimitable compoſitions of the laſt and preſent age would either ſoon become unintelligible, or the arduous taſk of learning both methods of notation would be impoſed on all ſucceeding generations. I am not however for the total excluſion of the term *ſulphuret*; let it be employed to dénote the compoſition

ſition of ſulphur with any baſis *in general*, whether alkaline earthy, metallic, oleaginous, ſpirituous or carbonaceous; in this moſt extenſive ſenſe the old nomenclature ſupplied no term, and yet ſome name was wanting; in this ſenſe therefore it may be retained.

Another general maxim advanced by M. Morveau is, that "the denomination of a chymical compound is neither clear nor "exact except it expreſſes by names conformable to their nature "the ingredients that enter into that compound." This maxim, unhappily too eaſily adopted by the French School, tends to the ſubverſion of the received language of all ſciences, and even of common life. By this rule we are to baniſh the name *water*, and inſtead of it ſubſtitute its component ingredients *hydrogenated oxygen* or *oxygenated hydrogen* *; and inſtead of *ice* we are to ſay *decaloricated*, *hydrogenated oxygen*, and for *ſteam*, *caloricated hydrogenated oxygen*. Inſtead of common *ſoap* we are to ſay *oleaginated ſoda*, and for *glaſs*, *ſilicited alkali*, &c.

The French chymiſts it is true retain the name *ſoap*, but in contradiction with their own principles; for they ſurely cannot in this word diſcover the radicals of its compoſition. (Nay Lavoiſier

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* Fourcroy expreſsly ſays water may be called *oxide* of *hydrogen*. See St. John's method of nomenclature, p. 64.

retains the old word nitre and ſalt peter conjointly with that of *nitrat* of *potaſh*, ſee his Treatiſe of Chymiſtry, vol. I. p. 79 and 232, French) and yet they diſapprove of my retaining a few of the antient denominations that were as well known to all chymiſts as the names *ſoap* or *glaſs* in the language of common life, ſuch as *Glauber*, *Epſom* and *Sylvian* (rejecting only the term ſalt uſually tacked to them; but evidently ſuperfluous) alſo ſelenite, gypſum, borax and alum, theſe I retain for two reaſons, firſt, becauſe they expreſs their reſpective objects by a *ſingle* name, which appears to me a conſiderable advantage, and ſecondly, becauſe thoſe names continually occur in all treatiſes of chymiſtry publiſhed before the year 1790, and in many ſince, and conſequently muſt be known by all who wiſh to underſtand them.

Morveau and the French School in general reject the names of inventors "as having no conformity either generic or individual with things," and for the ſame reaſon they ſhould reject the names *Alexandria* and *Conſtantinople* derived from the founders, and in general, by the ſame rule, all names of places ſhould be changed for ſuch as would expreſs their ſituation. Is it not therefore evident that when the ſignification of names is already fixed and generally known, that they ſhould be retained*,

* 19 Rox. p. 374.

tained, the inconvenience of rejecting them being far ſuperior to any advantages propoſed by the change? yet ſtrange to tell they object to a few new names which I introduced in mineralogy though exactly founded on their own principles and not attended with any inconvenience, as they denote objects not known by any denomination previouſly aſſigned to them, and even poſſeſſing peculiar properties of great importance. Thus they diſapprove * of the name *muricalcite*, a name which I thought proper to beſtow on ſuch limeſtones as contain a notable proportion of magneſia, and *baryto calcite* on ſuch as contain a notable proportion of barytes, theſe ſtones having been diſtinguiſhed by no name, and theſe new names being in conformity with the principles of the French ſchool derived from and grounded on their component ingredients.

Mr. Tennant has ſince ſhewn in an important paper in the Philoſophical Tranſactions for 1799 the evident neceſſity of diſtinguiſhing theſe *muricalcites* from common limeſtones with which they have always been confounded; and from the deleterious properties which barytic lime water poſſeſſes it is evident that the limeſtones that contain any proportion of barytic earth cannot without danger (ſometimes to human life) be

* 23 Ann. Chym. p. 103.

be confounded with common limeſtones. The ſtone which the Germans without any inconvenience called *apatite*, approaching in our language too nearly the word *appetite*, I have called *phoſphorite* to mark its compoſition; but as the phoſphoric acid has alſo been detected or at leaſt ſuſpected in compoſition with argil, to diſtinguiſh this compound from the former I call it *phoſpholite*, a name better ſuited to it than that of *valentia garnet*, by which it was formerly denoted, a change attended with no inconvenience, as the ſtone itſelf was known in no part of Europe but Spain. To theſe denominations they object their *monotonous terminations*; but theſe terminations are beſt ſuited to our language; they do not recollect that the terminations in *at*, as *nitrats*, *ſulphats*, *muriats*, *carbonats*, *phoſphats*, *oxolats*, &c. &c. are equally monotonous in theirs. They think, or rather magiſterially decide, that minerals ſhould be denoted by the ſame name, as ſimilar chymical compounds; a rule that might be admitted if ſuch minerals were not previouſly generally known by other ſingle appellations, and if their compoſition was perfectly ſimilar to analogous chymical compounds. But in the firſt place ſeveral minerals are already generally known by peculiar proper denominations, for inſtance, *cinnabar*, ſurely a more convenient name than the drawling new name *ſulphurated red oxide of mercury*, and in the next place they ſhould conſider that chymical compounds being artificial productions may

may contain only thoſe ingredients which their compound names import. But minerals whoſe principal compoſition is analogous, often contain other ſubſtances alſo, which the new chymical name would not expreſs, and would thus lead to important miſtakes. Thus, for inſtance, *phoſphorite* is commonly contaminated with ſilex, aerated lime, muriated lime and iron, and ſometimes with manganeſe and fluor; whereas the analogous chymical compound on which the name phoſphorated lime is properly impoſed, is free from ſuch contaminations. Subſtances therefore ſo different ſhould certainly be diſtinguiſhed by different names, or at leaſt the word *natural* or *native* muſt be tacked to the name of the chymical compound; and though with reſpect to foſſile metallic chymical compounds this inconvenience may often be avoided by the addition of the word *ore*; yet here alſo the antient name often expreſſes ſome other inherent property, thus the term *vitreous* added to ſilver ore denotes not only the compoſition but alſo the eaſy fuſibility of that particular ore.

Another inconvenience ariſing from denominations aiming to expreſs the compoſition of the objects on which they are beſtowed, is, that they often cannot expreſs the proportion of the compounding ingredients without conſiderable embarraſſment; this proportion nevertheleſs is a circumſtance often of great importance, as it induces an important difference in their properties, and the embarraſſment is greater when the compounding ingredients are numerous.

numerous. To obviate the firſt, in one particular inſtance, the French ſchool have very properly aſſigned different names to compounds holding different proportions of oxygen and poſſeſſing different properties in virtue of that difference of proportion, as *acids* and *oxides*; but ſeveral of the vegetable acids differ from each other only in the proportion of ingredients, which cannot, even if fully known, be expreſſed in detail on every occaſion. So to obtain a diſtinct knowledge of the various combinations of ſulphur or ſulphur and hydrogen with different baſes is of the utmoſt conſequence to any one that wiſhes to obtain any inſight into the phænomena preſented by mineral waters during their analyſis, or of the nature of other ſulphurated compounds; on this obſcure and intricate inveſtigation, moſt certainly the eminent abilities of Fourcroy, and the conſummate ſkill and ſagacity of Berthollet and his aſſociate Welter have thrown the cleareſt light, yet I muſt own that the terms employed by the two laſt to denote the different compounds appear to me very perplexing, though the beſt that could be choſen on the principles of the new nomenclature.* I flatter myſelf therefore that a ſhort explanation of them, and of the names I would wiſh to ſubſtitute in their room, will not be unacceptable nor out of place on this occaſion.

Sulphure,

* See 25 Ann. Chym. p. 230. and 3. New. Rox. Jour. p. 436.

Sulphure, Sulphuret of the Engliſh.

By this the *French* underſtand the compound of *ſulphur ſingly* with any baſes except hydrogen; theſe compounds I call *hepars* if unmetallic, but if metallic I apply their antient names.

Hydrogene Sulphuré, Sulphurated Hydrogen.

This complex denomination expreſſes the union of ſulphur with hydrogen. I call it by its antient name, hepatic air.

Souffre Hydrogené

This term expreſſes the union of ſulphur with hepatic air, the ſulphur abounds and it does not form a permanent air, but aſſumes an oily form. Engliſh neologiſts would, I preſume, have called it *hydrogenated ſulphur*; it is plain that this name does not expreſs all the ingredients, for it is to hepatic air and not merely to hydrogen that the ſulphur is united; I call it therefore *hepaticated ſulphur*.

Hydro Sulphure.

Hepatic air is capable of uniting to various baſes, and even precipitates ſulphur from them if previouſly united to them; on theſe compounds Berthollet beſtows the above denomination. The Engliſh, I ſuppoſe, would call them *hydroſulphurets;* I call them *hepatules.*

Sulphure Hydrogené

This denotes the union of hepaticated ſulphur (ſouffre hodrogené) with any baſis; the Engliſh, I ſuppoſe, would call theſe compounds *hyrogenated ſulphurets*; this appellation does not expreſs the compoſition. I employ the term *hepaticated* to expreſs this compoſition. Berthollet has diſcovered that fixed alkaline hepars when diſſolved, or even moiſt, are always in this ſtate, and conſequently that an alkaline hepar cannot exiſt but in a dry ſtate.

In the caſe before us we ſee the confuſion and perplexity occaſioned by a ſtrict adherence to the rule recommended by Lavoiſier,* that a conformity or connexion ſhould be maintained between the names of a baſis and its compounds; in ſome caſes it is highly proper, and hence the different degrees of oxygenation of certain acids are very properly indicated by a ſlight alteration of the termination of the name of each, as *ſulphureous* and *ſulphuric*; but theſe names indicate only *extreme ſtates*, the firſt only the ſmalleſt degree of oxygenation conſtituting an acid, and the laſt the ſtate of *perfect ſaturation*; now this ſame acid commonly occurs in neither of theſe ſtates, but in a ſtate participating more or leſs of both; theſe names therefore applied to it when thus circum-

ſtanced

* Lavoiſ. p. 72 and 73.

ſtanced are falſe, and as in this intermediate ſtate it has always been known by the name of *vitriolic acid*, I think this name ſhould ſtill be retained. Nay the ſulphureous acid is itſelf capable of two very different ſtates, as may be ſeen in the 6th vol. of De Machy's Edition of Junker, p. 143. So the term *nitric* is very proper to denote the full ſaturation of the nitrous baſis with oxygen. But the term *nitrous*, employed to denote the ſmalleſt degree of oxygenation neceſſary to convert this baſis into an acid, is improper, as it has evermore conveyed a different idea. Hence I expreſs this loweſt extreme of oxygenation by the term *mephite nitrous*, as the radical primary baſis may be called *mephite* or mephitic air, inſtead of the new coined name *azote*. And the term *nitrous acid* may ſtill denote, as it has evermore done, the mean ſtate of oxygenation, that indeed in which it is uſually found, and for which the French School have no name but that of one or other of the extreme ſtates, which muſt therefore be falſely applied. The term *epinitrous air*, introduced by the learned and ingenious Dickſon, may be uſed to denote what Doctor Prieſtly called *dephlogiſticated nitrous air*, at leaſt until its nature is better developed.

It is in vain that the authority of Bergman is invoked to countenance the ſuppreſſion of the antient names of Glauber, Epſom, &c. rejecting only the oſtentatious additions made to ſome of them, as *ſal admirabile Glauberi*, &c. 4 Bergm. p. 257. It is true

he affirms that *the beſt names are ſuch as indicate the compoſition or ſome eſſential property*, but of this ſort he gives no example, nor rejects any old names merely on this account; on the contrary where *expreſſive names* cannot eaſily be had, he tells us it is better apply ſome that convey no determinate expreſſion, p. 259, and this is often the caſe where different proportions or numerous ingredients are to be denoted.

Hence I am far from rejecting, but on the contrary applaud the ingenuity of the inventors of the terminations of *at* and *ite* to denote the different proportions of oxygen in the acids contained in different compounds, as *ſulphats*, *ſulphites*, *nitrats* and *nitrites*, &c. as ſuch general names were undoubtedly wanting and the old ſchool afforded none. But the welcome admiſſion of theſe does not require nor imply the diſmiſſal of ſuch of the old as were faultleſs and enfranchiſed by preſcription.

An highly valued friend ſuggeſted to me, that the uſe of the old names was a departure from the ſyſtem on which the new denominations were founded. I replied that ſyſtems were the creatures of convenience, and ſhould be adhered to only as far as they promoted it; nitre, epſom, borax, &c. are much ſhorter (and equally well known) than nitrated pot aſh or ſulphat of magneſia, and Lavoiſier himſelf preſerves the name borax; he might as well have preſerved that of epſom.

The

The 5th rule (very properly) laid down by Morveau with reſpect to names is, that they ſhould be adapted to the genius of the language; conſequently if old names be retained they ſhould be employed in the true uſual ſenſe of their known ſignification in that language, and neither extended beyond it nor reſtricted within narrower bounds; on this ground I reject the term *potaſh* employed to denote the vegetable fixed alkali in its pureſt ſtate; for that name both in Engliſh and French has always denoted an impure alkali, but the pureſt alkali of this ſort having been formerly denoted by the name of *ſalt of tartar*, a name certainly improper, I ſubſtitute in its room the unexceptionable name *tartarin*, which by its affinity to the former eaſily ſuggeſts its ſignification, and is moreover attended with a ſmooth flowing adjective *tartarinated*, which is often wanting. For the ſame reaſon I reject the name *ammonia* to expreſs the *volatil alkali*, as the name ammoniac has always been employed to expreſs the combination of a volatil alkali with an acid, and if no particular acid was expreſſed the muriatic was underſtood; inſtead of volatil alkali, which is a compound denomination, I ſubſtitute *volalkali*, whoſe ſignification cannot be miſtaken. Its adjective is not indeed quite ſo happy; inſtead then of *volalkaliſed*, I uſe the word *fuliginated*, which eaſily indicates the ſame idea. Theſe are the only new chymical names I employ.

The term *oxide* is alſo unſuited to our language, in which it naturally expreſſes the *hide of an ox*. In pronunciation they cannot

cannot be diſtinguiſhed; in its ſtead I would uſe *oxat* or *oxidat*, and inſtead of *oxided* I would ſubſtitute *oxidated*. The application of either of theſe terms to metallic ſubſtances in an oxidated ſtate is generally ſuperfluous, as ſuch ſubſtances are already denoted and known under the name of metallic *calces*; Guyton * has lately proved that *diamonds* are the pureſt carbon, yet ſurely even the French School will not attempt to ſuppreſs that well-known name and exchange it for *carbon*. Neither, I ſuppoſe, will they call *charcoal* an *oxide of carbon*, though proved to contain ſome portion of oxygen; and for the ſame reaſon I ſhall not exchange the well-known term *plumbago* for that of *carburet of iron*, though with reſpect to ſimilar compounds of other metals the term *carburet* ſhould be employed.

This ſyſtem of conciliation the French School rejects with diſdain, Guyton tells us "it is ſo much more difficult to conceive as it is an evident ſacrifice of principles to habits," § as if the ground of their ſyſtem of nomenclature were univerſally allowed, and afforded rules ſo ſtrict and general as to enjoy the ſingular privilege of admitting no exception! as if there was no ſuch thing as *principles of convenience*, or if there were ſuch principles,

* It is with much regret and reluctance I mention this gentleman under this new name, as he was generally known and gained immortal fame under that of *Morveau*; hence I ſhall ſtill uſe this in quoting his former works.

§ 25. Ann. Chym. 207.

ciples, that they were to be ſacrificed to mere ſpeculative truths (if truths) of much leſs importance; a truly harſh, intolerant and deſpotic maxim! as ill calculated to point out the moſt advantageous road to ſcience, as the maxim that a ſtraight line ſhould always be followed, would be to inſure us the beſt road to the ſummit of a mountain, though preſenting in that direction a ſeries of ſcabrous and abrupt precipices; and hence departed from by Lavoiſier himſelf, as we have ſeen in the inſtances of *ſalt petre* and *borax*, and indeed by the whole French School in the inſtances of *water* and *diamond*, as already mentioned: For common ſenſe in ſome inſtance or other ſeldom fails of aſſerting its rights, yet Lavoiſier tells us he was cenſured (by ſome chymical bigots) for this condeſcenſion. The principles of religion and juſtice are the only that can in no poſſible caſe yield to conveniency.

Among many juſt reflections that occur in the preface to Lavoiſier's celebrated elementary treatiſe of chymiſtry, there are ſome connected with this ſubject that appear to me not quite correct. Thus, p. x. (of the original) he tells us "that the only "way of avoiding theſe errors (unfounded hypotheſes) is to ſup- "preſs reaſoning, or at leaſt to ſimplify it as much as poſſible, as "it proceeds from us, and can alone lead us aſtray; to try it al- "ways by the teſt of experiment, to preſerve only the facts, "which are the *data* given by nature and which cannot deceive "us; to ſeek for truth only in the natural concatenation of ex-

"periments

" periments and obſervations, as mathematicians arrive at the ſo-
" lution of a problem by the ſimple arrangement of the data, &c."
From this paragraph we might be led to conclude that all reaſoning ſhould be baniſhed from chymical inveſtigation, or at leaſt, that only the ſimpleſt ſhould be admitted; yet it may eaſily be ſhewn that the moſt ſignal inſtances of ſucceſsful chymical inveſtigation in the obſcureſt ſubjects, and the happieſt diſplay of chymical ſagacity, are the reſults of very complicated reaſoning. Such is Berthollet's theory of aqua regia, Berthollet and Welter's obſervations on hepatic air, Fourcroy's on hepatic waters, Vauquelin's theory of the mutual decompoſition of nitrous air and the ſolution of vitriol of iron,* moſt of Scheele's and many of Klaproth's Analyſes, and a few others.

This juſt arrangement to which mathematicians owe the eaſy ſolution of their problems is itſelf the reſult of profound reaſoning, as is evident in the formation of equations. But the modes of reaſoning employed in the ſolutions of mathematical and chymical problems cannot properly be compared, the former being founded on the relation of identity or equality, and the latter on that of cauſe and effect.

* All foreign chymiſts are infinitely obliged to him for giving the old denominations of weights and meaſures inſtead of *centimetres*, &c. Chymiſtry aims at enlightening the world, and not Frenchmen alone; it ſhould therefore ſpeak a language univerſally underſtood, and ſhake off the yoke of national pedantry.

PAGE vi. he tells us " it is the ſeries of facts that conſtitutes " ſcience." I ſhould rather ſay it was a knowledge of the relation that ſubſiſts between the facts that occur; but neither the facts themſelves can often be diſcovered without much ſubtle reaſoning, nor can they be marſhalled in a luminous ſeries without diſcovering the reciprocal relations of the component ingredients of compound ſubſtances to each other, a diſcovery which often requires an elaborate train of reaſoning.

LASTLY, both he and Morveau tell us that the memory of learners is ſingularly relieved by compound denominations expreſſing the component ingredients of each compound. In reply to which, I ſay, that the ſcience is not to be charged with a cumberſome train of words merely to gratify the indolence of beginners; are we then on every occaſion to ſubſtitute the definition of words for the words themſelves? are we, in imitation of the Germans, to ſay a *handſhoe* inſtead of a *glove*? Helvetius has long ſince remarked, that every man of common underſtanding poſſeſſes ſufficient power of memory to retain the ſignification of moſt words in his own, and often of thoſe of ſeveral other languages; chymiſtry and mineralogy together ſcarcely preſent two hundred appertaining to them alone; neither the ſciences of aſtronomy, law or medicine afford fewer.

THE faſhionable rage of coining new words from the Greek, *without any neceſſity*, has been particularly baneful to mineralogy,

inſomuch that foreigners, though well acquainted with the received terminology, cannot without being verſed in that language underſtand the meaning of the new-fangled terms lately introduced. It is well known that the mineralogical knowledge of all Europe is chiefly derived from the Germans and Swedes, whoſe nomenclature is in moſt inſtances the ſame, and where any ambiguity has ariſen it has been removed by the exertions of Werner. His nomenclature, where not too diſcordant with the language or at open variance with the received technical names of other countries, ſhould therefore for the ſake of preciſion and uniformity be univerſally preſerved.

Abbe Hauy, who is now preparing a treatiſe of mineralogy, of which the higheſt expectations are juſtly entertained, ſince, in addition to his own ſuperior intelligence and profound phyſical knowledge, he is aſſiſted with the chymical abilities of Guyton, Vauquelin, Deſcotilles and many others, and the extenſive reſearches of Dolomieu, ſeems convinced of the propriety of retaining the received terminology, at leaſt for the preſent, with the limitations and reſtrictions above-mentioned; for in his prefatory diſcourſe, 27 Journ. des Mines, p. 224, he tells us, " that " with reſpect to minerals of the firſt claſs (that is, conſiſting of " mere earths) he left them thoſe names which they had hitherto " borne, and did not take the liberty of impoſing new ones, " except in caſes of neceſſity, as where a new ſpecies occurred, " either formerly unknown, or comfounded with one of a different " nature.

" nature. We have altered ſuch names only as were intolerably " ambiguous."

YET after this declaration we find the following new-coined names:

Teleſia—for oriental rubies, ſapphires and topazes. Thus ſapphire is called *blue teleſia*, &c.

Cymophane—for the chryſoberil of Werner.

Amphibole—for cryſtallized hornblende.

Pyroxene—alſo cryſtallized hornblende; at leaſt I have reaſon to think ſo, for of the eleven external characters of both given by La Metherie, there is only one in which they abſolutely differ, that is, the *electrical*.

Staurotide—for ſtaurolite, the name very properly impoſed by La Metherie inſtead of the compound name it bore before.

Axinite—I believe to be only a variety of thumerſtein.

Actinote—for actinolite, the termination *lite* moſt properly denoting a ſtone.

Thallite—for delphinite, the name already given it by Sauſſure.

Idocraſe—ſeems, as I conjecture (ſeeing no exact account of it) a variety of olivin.

I PASS over ſeveral new names, as *euclaſe*, *dioptaſe*, *chabaſie*, &c. as they may poſſibly denote new ſpecies, which I wiſh were ſettled in conjunction with Berthout or Van Buch, or ſome of the Wernerian School. Yet even theſe denominations we are told are only *proviſional*, being hereafter to be altered as analyſes may require.

I MUST here add, by way of note, that Citizen Guyton, in reviewing the firſt volume of my Mineralogy, 23 Ann. Chy. p. 105 and 106, has fallen into two miſtakes which I am perſuaded his candour will prompt him to acknowledge. The firſt is, in ſtating that my experiments on the fuſibility of different ſtones and earths were made in limeſtone veſſels, whereas they were in Heſſian crucibles. The ſecond is, in ſtating the alleged infuſibility of barytic earth and lime as the reſult of my experiments, whereas I expreſsly quoted Lavoiſier, having myſelf made no experiments on ſuch mixtures, (See Mem. Par. 1783, p. 599 and 600.) Other writers have frequently imputed to me miſtakes of Bergman, though I expreſsly quoted him.

Fig. 1.

Fig. 2.
Half Size.

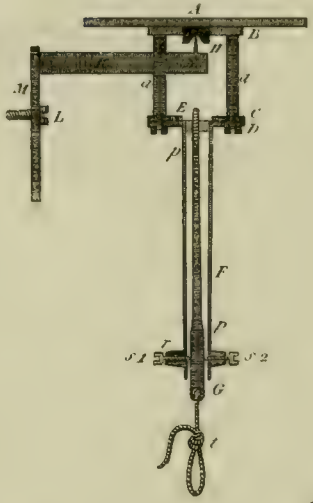


Fig. 3.

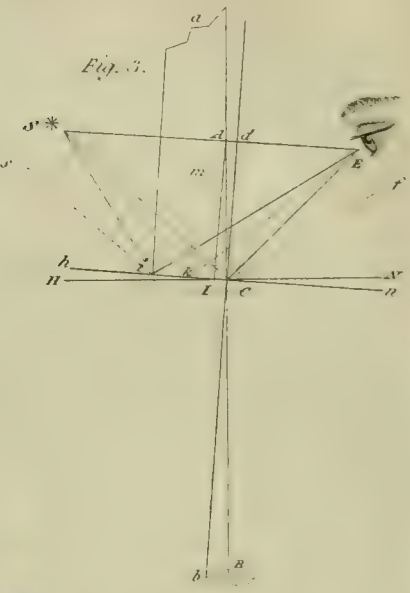


Fig. 4.

Fig. 5.

Fig. 6. ½ Size

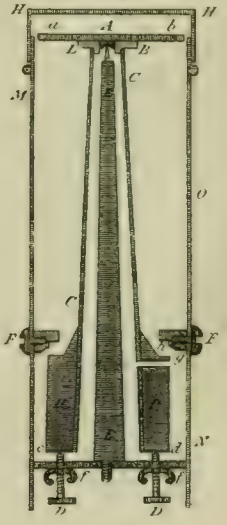


Fig. 7.

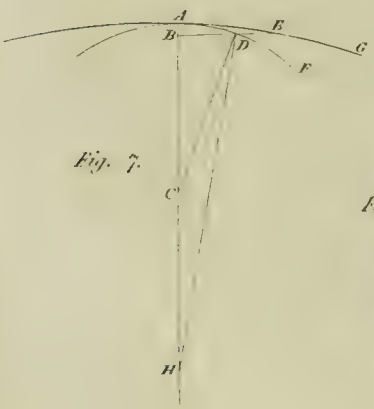


Fig. 8.

J.L. del.

A DESCRIPTION *of a* REFLECTING LEVEL *or an* ARTIFICIAL HORIZON *for taking* ALTITUDES *of the* CELESTIAL BODIES, *&c. on Land by* HADLEY's QUADRANT, *with ſome* REMARKS *on different* LEVELS. *By the Reverend* JAMES LITTLE.

Read October 20th, 1798.

THE uſefulneſs at ſea of that excellent inſtrument, the Hadley's quadrant, is known to every navigator, it being the only one by which, without any ſtand or fixed poſition, and even when the obſerver is in motion, accurate obſervations can be made of the altitudes of the celeſtial bodies and their angular diſtances in order to find the longitude; and it would alſo, as being the moſt accurate and ready of all ſmall and portable inſtruments of that nature, be exceedingly uſeful at land, eſpecially to a traveller, in taking curſory obſervations, to diſcover the latitude, &c. of places occaſionally viſited, if it were furniſhed with an artificial horizon or level to ſupply the place of that natural one which at ſea the water around the obſerver affords, and from which the altitudes

altitudes of the celeſtial bodies are meaſured: for want of ſuch a level on land, they who make obſervations with an Hadley's quadrant, which is not fixed and adjuſted by a plummet or ſpirit-level, are obliged to take theſe altitudes by meaſuring the angular diſtance between the object and the image of the ſame ſeen on the ſmooth and level ſurface of a ſtagnant fluid, as water, quickſilver, &c.; and when the object is very remote, that angular diſtance will be double its elevation above the real horizon. But as ſuch obſervations muſt be made in the open air, and the leaſt breath of wind will ruffle the ſurface of the fluid, it is neceſſary to cover it with two glaſs planes joined at their upper edges and erected like the roof of a houſe over the veſſel containing the fluid: each of theſe planes muſt have its oppoſite ſurfaces exactly parallel, which is never the caſe with plates of looking-glaſs, and the proper planes with the requiſite apparatus are in few places to be procured, and liable to accidental injury. Inſtead of this fluid artificial horizon I have uſed the following one, which can any where be eaſily made, and which I found to anſwer well, as being accurate and portable, leſs liable to accidents in travelling than the former, and diſcovering in the act of obſervation its own errors of adjuſtment. I conſtructed it in the two forms hereafter deſcribed, the firſt of which is repreſented in a perſpective view in fig. 1, and in a ſection through the middle in fig. 2: the ſecond form appears in fig. 8, in a like ſection through the common axis of its parts.

It conſiſts of a circular plate of glaſs *A*, (made of a piece of looking-glaſs) whoſe upper ſurface is plane and poliſhed, but the under ſurface has its poliſh taken away by being rubbed with a piece of lead and flour of emery with water, ſo that an image of any object can appear by reflection from its upper ſurface only: to this is cemented with black ſealing-wax the frame *B C*, (made of ſteel, or it might be of braſs) both having been previouſly made very clean and bright and heated in an oven, then cooled ſlowly: this frame is compoſed of the round plates *B* and *C* faſtened together by the two rods or pillars *a a* ſtrongly riveted or brazed to both plates: the piece *D E F* is compoſed of the round plate *D*, which is exactly fitted and ſcrewed cloſe and immoveable by two ſcrews to the plate *C*, ſo that both may be parallel to *A* and *B*. In the middle of the plate *D* is brazed the pipe *p p*: this pipe is made of ſteel, and its ſides as thin and light as may be conſiſtent with ſtrength to ſecure it from being eaſily bent; it tapers a little towards its end, and its mean external diameter is about $\frac{3}{10}$ of an inch: in the middle of this pipe is a ſteel wire hardened towards the lower end and tapering upwards towards *E*, where it is ſcrewed (looſely) through the middle of a ſhort pin or plate faſtened in the mouth of the pipe, which pipe is (for ſtrength) inſerted quite through the plate *D*. The lower end of the wire, which extends a little below the pipe, has a hole through it at G at right angles to its axis, as repreſented (enlarged) in fig. 4, and alſo in fig. 5 in another ſection of the wire which is at right angles to the ſection in fig. 4: this hole is to contain the knot

knot of a looped thread *t*, which is inſerted through a very ſmall perforation made in the axis of the wire, and only ſo wide as juſt to admit the thread tight through it, or inſtead thereof a very fine and flexible ſtring of cat-gut or fiddle-ſtring, in order that when a weight is appended to the ſtring it may pull the wire only by a point in its axis. A ring or hoop *r r* (of iron) is brazed on the lower end of the pipe *p p*, juſt ſo broad as to admit four equidiſtant and oppoſite adjuſting ſcrews *s s*, (made of tempered ſteel, and having ſquare heads, in order to be turned by a key as well as by a turn-ſcrew) to be inſerted through it; and the hoop is to be ſo thick as to ſuſtain, without change of its circular ſhape, the preſſure of the ſcrews againſt the wire *G E*. The pipe *p p* is to be fixed perpendicular in the plate *D*, that it and alſo the wire *E G*, when in the middle of it, may be at right angles to the mirror; and then any little deviation of the wire from this will be corrected by moving the wire (which muſt be ſo fixed as to yield eaſily) towards that ſide where it makes too great an angle with the mirror, which is done by firſt withdrawing the adjuſting ſcrew on the ſame ſide and then advancing the oppoſite ſcrew: and when the wire is thus firmly preſſed by the four oppoſite ſcrews it cannot change its poſition after being made vertical to the mirror, nor conſequently the inſtrument alter its adjuſtment.*

* The plate *D* might be omitted, and the pipe with the wire *EG* might be faſtened in the plate C, were it not that there ſhould be a hole left in the middle of this plate to allow the needle-cap (hereafter mentioned) to be fixed in the plate *B*, and

In making this little inſtrument care muſt be taken to form it ſo, that when the frame *B C*, and its appendage *D F*, are ſcrewed together, then the common axis *A G* of the whole ſhall paſs through the centre of the plate *A* and be at right angles to its plane; for when this is the caſe, if it be contrived to ſuſtain the axis

and if made of ſteel, to be poliſhed anew, ſhould it ever contract ruſt; to prevent which it would be beſt to keep always a drop of oil in it. The wire ſhould be ſo thick at the place of the ſcrews that its diameter may be only about $\frac{1}{12}$ of an inch leſs than the internal diameter of the pipe, and when the inſtrument is adjuſted the wire ſhould be as nearly as poſſible in the middle of the pipe; for if it were removed too much to one ſide, each pair of the ſcrews, whoſe ends are all equidiſtant from one another, would not preſs the wire in points of it diametrically oppoſite; in which caſe the withdrawing of one ſcrew alone might not permit the motion of the wire to that ſide, but it might be requiſite to caſe two contiguous ones, and create a neceſſity for altering all the ſcrews, which would render the adjuſtment very troubleſome. This will be avoided by making the wire as thick as it may conveniently be juſt at the place of the ſcrews, and by having the pipe fixed at right angles to the mirror.

Another method of appending the weight to the ſtem is repreſented enlarged in fig. 6. The lower part of the ſtem is formed as at *S T*, and a pin P made of hardened ſteel is ſcrewed into it, in whoſe end is a conical cavity with a fine apex to receive the fine and hard point of a hook *H*, made of a thin plate of ſteel and faſhioned as in the figure: through a hole in the hook at *h* a ſtring is put to ſuſtain the weight, which it is plain will thus pull the ſtem only by that point in its axis which is the vertex of the conical cavity. Whether this or the above method of ſuſpending the weight is the beſt, I can't ſay, though I have tried both; but would rather recommend the former as being more ſimple, and conſequently fitter for a portable inſtrument, which ſhould conſiſt of as few parts and common materials as poſſible, leſt any ſhould be loſt, deranged, or damaged. I made trial of a ſmall ring (to hang the weight from) paſſed through the end of the ſtem which was made thin, but it would not anſwer.

axis *A G* in a vertical poſition, the plate *A* will be kept in an horizontal one, and will be a reflecting level: this is effected by ſuſpending the whole from a point *N*, (fig. 2.) in the axis, as near the plate *A* as can be, and hanging from the looped ſtring *t*, by a hook, a weight to ſwing in a veſſel of water, or in the air if it be calm enough. The point of ſuſpenſion is in the axis of a pin of ſteel *H** (tempered very hard) inſerted through and fixed in the plate *B*, having a conical cavity in it, whoſe apex is poliſhed to receive the point of a ſtrong and hard tempered ſteel needle *N*, which needle ſupports the inſtrument, and is itſelf ſuſtained perpendicularly in an iron arm *K*, riveted in the plate *M*, by which it is faſtened by the ſcrew *L* on the inſide of a veſſel or caſe about eight inches deep and near three inches wide internally: the arm *K* has ſuch a length and poſition as that the needle *N* ſhall be in the middle of the veſſel: this veſſel is to reſt or be ſuſtained ſteady on ſome ſtand, or to be ſteadily faſtened by a ſcrew in an upright ſituation to a firm door-poſt, tree or wall, &c. when the veſſel is vertical its upper edges muſt be level, and the arm *K* muſt be placed ſo high as that the upper ſurface of the plate or mirror ſhall be very little higher than the edges of the veſſel: the lower part of the veſſel is to hold water ſufficient-

* A ſtrong agate needle-cap, ſuch as is uſed for compaſs-needles and fixed in the plate *B*, would anſwer beſt.

ſufficient to cover the weight, and the upper part is to have a door in the ſide to allow the inſtrument to be ſuſpended and its weight hung by a hook from the loop of the thread *t*, or its vibrations to be ſtopt; for which purpoſes the door ſhould deſcend to near the ſurface of the water: it is to be ſhut at other times to prevent the wind from agitating the level.

From this deſcription it is evident, that when the inſtrument is ſuſpended from the point *N*, under the centre of the mirror, and the weight appended to *G*, or the middle point of the hole in the axis of the wire, theſe two points would be always in the line of the direction of gravity if the inſtrument itſelf had no weight, or if that line paſſed through the centre of gravity of the whole machine; but if the glaſs plate and metal frame could be divided into two parts, of unequal weight, by a plane paſſing in any direction through *A G*, then the axis *A G* would be thrown out of the direction of gravity, and conſequently the mirror out of the level more or leſs as the weight appended at *G* is leſs or more: this can ſcarce happen if the point of ſuſpenſion *N* be in the centre of the plates *A* and *B*, each of the plates *B*, *C* and *D* be of uniform thickneſs, the pillars *a*, *a*, of equal ſize, equidiſtant from *N*, diametrically oppoſite and at right angles to the plates *B* and *C*, and the ſtem *E F*, in the middle of the plate *D*, and perpendicular to it; and thus the line *A G* be the vertical axis of the whole and of each of the parts: this

eaſily may, and muſt at firſt be nearly effected; which will be known in ſome meaſure by the plate *A* reſting level when the inſtrument is ſuſpended on the needle *N* without any weight hung from the end of the ſtem; and if it could be effected accurately there would be no uſe for the pipe *p p*, and a ſolid ſtem *E F* might be fixed in the plate *D*; but when the weight is hung from the ſtem and the inſtrument receives a circular motion round the plumb-line (which certainly may be conſidered the ſame as *A G*,) it will be found that the image of a fixed object ſeen by reflection in the mirror, by an eye kept alſo fixed or viewing it through a fixed ſight or vane, will not during the motion of the inſtrument appear every where at the ſame diſtance from the centre or edges of the circular plate or mirror, but the image will move or ſhift its place in it; a certain proof that this was not horizontal, and that though the line *A G* is inſenſibly different from the line of gravity, yet that it was ſcarce poſſible to fix that line at right angles to the plate, but that there was a neceſſity for adjuſtment: for this purpoſe therefore the four adjuſting ſcrews *s s* were added, whoſe mode of action and effect have been explained.

If during almoſt half a revolution of the mirror round the axis *N G*, (which motion the arm *K* being made purpoſely thin and the pillars *a a* ſmall will allow) the reflected image of a fixed

fixed object ſeen as above-mentioned appears alſo fixed, the mirror is as perfectly horizontal as the plumb-line is vertical; ſuppoſing the ſurface of the mirror to be a true plane, (and one muſt be unlucky indeed in the choice of a piece of looking-glaſs if it be not ſufficiently ſo); and as it can by the adjuſting ſcrews be regulated to as much preciſion as the eye, even aſſiſted by a teleſcope, can aſcertain the contact of two images, and alſo proves its own adjuſtment, (by turning it round on its axis), which can be farther verified by comparing angles taken by reflection from it with thoſe taken from the ſurface of a fluid; alſo as it does not ſeem likely to change its form after being adjuſted and the ſcrews made firm; it ſeems in this reſpect unexceptionable, and it may be adjuſted the way before-mentioned by obſerving the image of any fixed object, as the ſun or moon, the top of a mountain or building, a candle placed at the diſtance of ten or fifteen feet, a wafer faſtened on the glaſs-pane of a window, &c. if when the mirror is made to revolve round the axis *A G*, without any pendulous vibration of the weight, the image *firſt aſcends* towards the part of the mirror moſt remote from the eye, and *then deſcends*, that part of the mirror in which the image is ſeen moſt elevated or culminating alſo aſcends or is raiſed above the level; and it will be lowered by firſt unſcrewing a little the adjuſting ſcrew which is neareſt to the moſt elevated place of the image on the ſame ſide, and then

ſcrewing

ſcrewing up the oppoſite ſcrew; for this will make the end of the ſtem approach towards or make a leſs angle with the mirror on the ſide where that angle was too great, i. e. where the mirror was too much elevated: alſo if the image, during the revolution of the mirror, appears *firſt to deſcend* towards the obſerver, and *then to aſcend*, that part of the mirror in which the image is loweſt is too much elevated above the horizon, and it will be depreſſed by withdrawing the ſcrew next to the loweſt place of the image, (which is now the higheſt part of the mirror) and advancing the oppoſite ſcrew as before; ſo that the adjuſter muſt be cautious not to miſtake *the order* of the aſcent and deſcent of the image, nor the *direction* of its *motion*; for as it has (in every complete revolution) both an aſcent and deſcent towards and from the higheſt and loweſt points, if the obſerver miſtakes the one for the other he may move the ſcrews erroneouſly, ſo that he muſt previouſly aſcertain the higheſt and loweſt place of the image.

That it is by the elevation of the mirror on the ſide in which the image appears it is made to aſcend, may be ocularly demonſtrated by giving that diameter of the mirror, which is in the ſame plane of reflection with the eye, the object, and image, an angular motion in that plane upon the point of ſuſpenſion; but if one would chooſe to know the optical reaſon of this

this effect and of the preceding directions, (and the knowledge of the theory of any inſtrument enables the reader to appreciate its merits, and alſo precludes injudicious attempts at alteration or improvement; on which accounts I think the theory ſhould accompany the deſcription of every machine); he may underſtand it from conſidering the following circumſtances; and indeed ſince differences in the diſtances of the near objects, by which alone this mirror, when not level, could ſometimes be adjuſted *, will produce a variety of motions and places of the reflected image, while the mirror is turned round the plumb-line, even when the altitude of the objects does not vary, and *vice verſa*, differences in the altitudes when the diſtances are unvaried; which might puzzle and diſguſt the perſon adjuſting it, and create doubts of its accuracy, it is the more neceſſary to give ſome explanation of its optical properties when it is propoſed for uſe.

In

* Though the want of adjuſtment of this level may be diſcovered in every obſervation of the celeſtial bodies, in which caſe their great diſtance renders the place of the eye, and conſequently of the quadrant uſed, immaterial; yet it can ſcarcely be adjuſted by an obſervation of the ſun, &c. unleſs they are very near the meridian, when there might not be time enough to accompliſh it, and alſo unleſs the altitude of the ſun, &c. be not too great: therefore the ſun will not anſwer well for adjuſting it in low latitudes, nor the moon except when at full: it will on theſe accounts be moſt generally convenient to adjuſt it by a near object, in which caſe the eye muſt be fixed, and then there will be a greater variety in the motions of the reflected image on the mirror, according to the relative diſtances of the eye and object from it; and there ſhould be an attention to more circumſtances in the proceſs of adjuſtment: wherefore I thought it proper to give ſome account of theſe particulars.

In fig. 3, let the line *C B* repreſent the ſtem of the level, kept by the plummet in a vertical ſituation: if the inſtrument were truly adjuſted, *C B* would be the axis of the mirror and at right angles to its reflecting ſurface; but if it be inclined to this ſurface, then no diameter drawn on it will be at right angles to *C B* but one; all others will be inclined to it in various angles of obliquity, the greateſt being that at right angles to the diameter which has no inclination; and if the mirror revolve round the line *C B*, this latter diameter will deſcribe a circle, but the others intermediate between theſe two will deſcribe oppoſite conical ſurfaces, having all of them different angles at their common vertex *C*. Let *H N* be that diameter at right angles to *C B* the plumb-line, and conſequently in the horizon, and let it be in that plane in which the eye, fixed at the place *E*, ſees the image of a luminous point *S* in the point *C*: from the law of reflection, the angle *S C H*, made by the incident ray *S C* with the mirror, muſt be equal to *E C N*, made by the reflected ray *C E* with the ſame; let now the mirror revolve about the axis *C B*, 'till another diameter, ſuppoſe that which is moſt inclined to *C B*, viz. *b n*, falls into the plane of viſion or reflection *S H C E*; the image of *S* cannot now be ſeen at *C*, becauſe the ang. *S C b* is not equal to the angle *E C n*, and it can be ſeen only in a part of the mirror where theſe angles at the point of incidence will be equal, and that will be (when the object is very remote) at a point *i*, whoſe angular diſtane *C E i*

from

from the point *C* is equal to twice *HCb* the obliquity of the ſpeculum*, or becauſe the angle made by two lines or planes is equal to that made

* When the object *S* is ſo diſtant that the ang. *C S i* is evaneſcent, the ray *S i* will fall on the mirror in the ſame direction as *S C*, that is, in *S i* parallel to *S C*: when the ray fell at *C* on the diameter *H N*, the angle of incidence was *S C A*, but in the change of the reflecting plane *H C* to *b C*, that ang. receives an increment *H C b* equal to *B C b*, and the ang. of reflection muſt receive the ſame; ſo that the angular velocity of the reflected ray is double that of the ſpeculum; and hence if the ang. *E C f* be made equal to twice *B C b*, *C f* will be the reflected ray, and if the eye were at *f*, it would ſee the image at *C*, but the eye remaining at *E* will ſee it at *i*, in a direction parallel to *f C*, and at an angular diſtance *C E i* equal to *E C f*, from the point *C*; becauſe there the angles of incidence and reflection *s i a*, *E i a* are equal, being the ſame as *S C d*, *f C d*. But if the object *S* be ſo near that the angle *C S i* or *s i S* is of a ſenſible magnitude, then the incidence *S i a* is leſs than it was before, by the quantity of this angle, and the angle of reflection *E i a* muſt be equally diminiſhed, or both receive equal and contrary alteration, which will be when the image is formed at that point *k*, between *i* and *C*, where the angles of incidence and reflection *S k m*, *E k m* will receive, the former ſuch an increment *k S i*, and the latter ſuch a decrement *k E i*, as will reſtore their equality. Hence it appears that if the diſtance of the eye remains unvaried, and alſo the elevation of the object *H C S* at the point *C*, the nearer the object *S* approaches this point, the nearer will the image approach to it; becauſe as the object accedes, the ang. *C S i* enlarges, and conſequently *C E i* muſt diminiſh.

But the various motions and poſitions of the image cauſed by the obliquity of the mirror to the horizon, cannot be clearly underſtood but by inveſtigating its path or place during a revolution of the mirror round the plumb-line. Let the line of direct viſion *E S* be drawn, *b C* be produced to *d*, and from the interſection *A* of *S E* and *B C* produced, let *A I* be drawn parallel to it, and from *i* let *i a* be drawn alſo parallel 'till it meets the axis *B C* produced, in the point *a*; ſince *b C* is perpendicular to the mirror, ſo alſo will be *I A*, *i a* (El. 11. 7.); and if the mirror be ſuppoſed to revolve round

made by two perpendiculars to the ſame, if Cb be perpendicular to bC, it will be equal to twice BCb; ſo that when this angle is nothing,

the line BA, carrying with it with its own angular motion the lines bd, IA and ia, theſe will continue parallel and perpendicular as revolving together in the ſame plane with the line bC, which has no motion with reſpect to the mirror; but the line AI, as interſecting the line of ſight ES, and perpendicular to the mirror, is always in the plane of reflection, therefore ſo alſo will the other lines parallel to it, and the image will always be ſeen in ſome part of the lines AI, ai; but theſe lines have a conical motion round the common axis BCa, and therefore the image will partake of that motion laterally as well as directly, during the revolution of the mirror from the plane (repreſented in the fig. by the plane of the paper) in which the diameter bC is that moſt inclined to the axis of motion AB; and the extent of the lateral motion will be limited within the angle CAI by the line AI and not by any other line ai, becauſe the former terminates both in the axis of motion and alſo in the line of direct viſion ES, and has the ſame angular motion about this line, which the plane of reflection has during the revolution of the mirror; ſo that when the mirror or the diameter bn has made $\frac{1}{4}$ of a revolution from its ſituation in the fixed plane $ESHC$, the image will have deſcended from the place i to the the diameter bn, paſſing through the axis AB, and now at right angles to its former poſition; and will alſo have receded laterally from that axis to the diſtance CI, for it will always be ſeen in the diameter bn (which is moſt inclined to the axis and wherein the line AI reſts) during its revolution; and its diſtance, as meaſured on the ſurface of the mirror, from the plane $ECHS$, will be the ſine of the angle made by the radius CI in its revolution; and its diſtance, the ſame way meaſured, from a plane paſſing through C and at right angles to the former, will be the coſine of the ſame angle and deſcribed by the radius Ci: and from this it appears that the path which the image traverſes on the ſurface of the mirror by the compoſition of ſuch direct and lateral motions during a revolution is an ellipſe, whoſe vertex is at i, its centre a point in the axis CA, its greateſt ſemiaxis the perpendicular diſtance between i and AC, and its conjugate ſemiaxis the perpendicular diſtance between I and AC, and that the apparent path or *locus* of the image is the ſame ellipſe ſeen obliquely by the eye at E, whoſe greateſt ſemiaxis is equal to the

ſine

nothing, the image will be at reſt while the mirror revolves round $B C$: if therefore the ſtem $B C$ be ſo turned by the adjuſting ſcrews, that

ſine of the angle $C E i$, $C E$ being the coſine; and leaſt ſemiaxis the ſine of the angle $C A I$, made to ſubtend an angle whoſe coſine is alſo $C E$.

Theſe ellipſes will be formed in any part of the mirror as well as the middle of it; becauſe the ſame change of poſition of its ſurface during a revolution which is preſented to the eye in whatever place it may view the image, will be the ſame: alſo the ellipſes will enlarge with the inclination or obliquity of the mirror to the axis of motion or ſtem of it, and their eccentricity will be inverſely as the altitude of the object, if the diſtance of the eye and object from the mirror does not vary; for the angles $C E i$ and $C A I$, determining the ellipſe, enlarge with the angle $B C b$ or $H C b$; but the lower the object is, the nearer does the line of ſight $E S$ approach the mirror, and the conjugate ſemiaxis of the ellipſe approaches to the vertex A of the angle $C A I$, within which it is confined; ſo that this axis, which is the ſine of the angle $C E I$, diminiſhes with the altitude of the object, while the angle $C E i$, equal to twice $B C b$, remains unvaried.

If the relative diſtances of the object and the eye from the mirror be ſuch, that the direct interval between the image i and the line $E C$ is but little greater than $I C$, the viſible place or path of the image will degenerate into a circle: if the image is at a leſs diſtance from $E C$ than this, its place will be again an ellipſe, whoſe tranſverſe and conjugate axes will have changed place with thoſe of the former one; but if the eye be at an infinite diſtance with reſpect to the object, the place of the image will be a right line at right angles to $b n$. Now from this enquiry reſult the following inſtructions:

1ſt. That ſuch an injudicious ſituation of the eye and object might be pitched upon for the adjuſtment of the inſtrument, that the image would appear to move in a circle; in which caſe it would be impoſſible to determine in what part the mirror was moſt elevated; ſo that the object choſen for adjuſtment ſhould be much more diſtant than the eye, and alſo not too much elevated above the level of the mirror;

 becauſe

that the point B will coincide with b (which is done by unſcrewing the ſcrew s 1, (fig. 2.) and ſcrewing up s 2,) then the plummet will bring back the ſtem CB from the place b to the line of gravity CB, and at the ſame time bring the diameter bC of the mirror to the horizontal poſition HN: and if the inſtrument be made with attention to the circumſtances already mentioned, the change of its centre of gravity (if any) from the plumb-line, made by alteration

becauſe then the elliptic path of the image will be more eccentric, and it will be eaſier perceived in what part of the mirror the vertex i of the ellipſe is formed, which it has been ſhewn is the moſt elevated part of it.

2dly. That the perſon adjuſting the mirror muſt make it revolve a full ſemicircle, in order that both vertices of the ellipſe may be ſeen; for this, by doubling the greateſt angle of aberration, may make it perceptible, though ſingly the motion and divarication of the images could not be obſerved: for which purpoſe the inſtrument muſt be changed on the needle, and replaced with its other ſide towards the arm K which ſupports it: and if the caſe or veſſel, in which it is ſuſpended, could be turned partly round, it would facilitate the adjuſtment when done by the image of a very remote object.

3dly. That an obſervation may be made accurately by this level, even when it has a great error of adjuſtment; by taking the mean of the *greateſt* and *leaſt* altitudes of the obſerved object, which are diſcoverable by the greateſt and leaſt divarication of the images during a ſemi-revolution of the mirror, which will be ſeen in that diameter of it, wherein is alſo the tranſverſe diameter of the ellipſe: and in it is to be taken one of the two altitudes, and, after turning the mirror half round, the other: the mean of theſe would reduce the reflected image to the centre of the ellipſe, and is therefore the true angular diſtance or altitude. This advantage of the level did not occur to myſelf, but was ſuggeſted by Mr. Profeſſor Brinkley, whoſe regard to the intereſts of ſcience inclined him to give much attention to this inſtrument.

alteration of the ſtem, will be ſo little as to be quite imperceptible; but were that change ever ſo great, ſince the adjuſtment is made by what is afterward actually the plumb-line, it can produce no error, provided the weight which is appended remains the ſame.

By this means the level may be adjuſted to a certain degree of accuracy, but never to any thing near preciſion; this can be effected by no means that I know, ſo well as by the Hadley's quadrant itſelf, to whoſe uſe it is ſubſervient, and from which in return it moſt readily receives its perfect adjuſtment; which is done by obſerving, when the image of a very remote object, as the ſun or moon or a bright ſtar in twilight, when near the meridian and their altitude not great; or the top of a very diſtant mountain ſeen in the mirror coincident or in contact with the image of the ſame object reflected by the quadrant, at the middle or edge of the tranſparent part of the horizon-glaſs, will preſerve (in the ſame place of that glaſs) its coincidence unvaried, while the level is made to revolve half a turn round its axis: if the image in the level riſes or falls from that in the quadrant, it muſt be adjuſted by the ſcrews as before. If the quadrant is furniſhed with a teleſcope that inverts, (by the help of which the adjuſtment may be rendered far more accurate than it could be done by the naked eye) then the aſcent of the image in the mirror, from the fixed image in the quadrant, muſt be regarded as its deſcent, and *vice verſa*. Any *near* fixed object, of ſuch

ſuch length in an horizontal direction as to be ſeen, when no teleſcope is uſed, in the ſilvered and unſilvered parts of the horizon glaſs, will ſerve for the adjuſtment, provided the eye be fixed, by having the quadrant immoveably placed on a ſtand, or faſtened to a board while the adjuſtment is performing; ſo that it may thus be very eaſily and accurately effected at any time even within doors, (care being taken that the floor of the room does not by ſpringing or bending change the relative altitudes of the level and object from the ground, and their angular diſtance) by fixing up as an object a ſtrip of black paper or any kind of foil, &c. on a pane of the window, extended horizontally: and a ſmall *fixed* teleſcope, furniſhed with croſs wires ſet diagonally in the tube, (that the horizontal image may appear within the angles at the centre) and the wires placed at ſuch a diſtance from the eye-glaſs, that a motion of the eye will not affect the apparent place of the image in the field of view, may be ſubſtituted inſtead of the quadrant, if it be more convenient to do ſo; the manner of performing this adjuſtment is deſcribed underneath*.

It

* Unleſs the adjuſtment by a *near* object be conducted in a judicious manner, a perſon may weary himſelf in doing it, where yet there is no difficulty. Let the quadrant be ſo fixed on a ſtand or rather to an upright board, on which is faſtened the arm *K* ſupporting the level, that the two images may by the motion of the index, &c. be brought to coincide near the middle of the mirror: the mirror may then be firſt turned round on the needle, 'till ſome certain diameter of it, as ſuppoſe that in the direction

It muſt be evident from this account, that no error can happen in any obſervation made with this inſtrument, when it can be uſed at all in the abſence of high winds, if its equilibration is permitted,

direction of the pillars *a a*, be in the plane of viſion or parallel to it, and the interval and poſition of the image in the mirror, with reſpect to that in the horizon-glaſs, ſhould be remarked: the weight being then taken off, the mirror ſhould be reverſed, and its oppoſite ſide placed over the arm *K*; and when the weight is replaced, the ſame diameter of it ſhould be brought into the plane of viſion as before: the image in the mirror will now be ſeen in its higheſt or loweſt poſition in this diameter as the place it appeared in before was the contrary; and the image in the quadrant ſhould be raiſed or lowered, by moving the index, 'till this image is judged to be in the middle between thoſe two places of the image in the mirror; and this latter ſhould then be brought, by altering the two adjuſting ſcrews, which are for this purpoſe placed directly under the pillars, to coincide with the image in the horizon-glaſs. By a few trials, reverſing the mirror, and altering the place of the images, 'till that in the quadrant is got into the middle between the extreme poſitions of the other image (which is the centre of the elliptic path it deſcribes) and this latter image then made to coincide with the former, the aforeſaid diameter of the mirror will be made level; and the diameter at right angles to it will be alſo rendered ſo, by turning the mirror about on each ſide and altering the other two adjuſting ſcrews as the conſequent elevation or depreſſion of the image directs. If a teleſcope only, furniſhed with croſs wires, be uſed for adjuſtment, inſtead of a quadrant, then the directions here given with reſpect to the image in the horizon-glaſs in the quadrant, are to be applied, as if ſpoken of the centre of the croſs wires of the teleſcope. A little practice will produce ſuch a degree of dexterity in this operation, that the adjuſtment may be effected in a very ſhort time; I have done it more than once (to the exactneſs of leſs than a minute of a degree) in leſs than ten minutes. The mirror will be more eaſily turned round its axis, without producing pendulous vibrations of the weight, and adjuſted, if the weight be hung by a thread about a foot long, faſtened to the looped ſtring *t*, and made to ſwing thus in water contained not in its own caſe, but in another veſſel, 'till the adjuſtment is finiſhed: then the weight and level may be hung as before.

permitted, by having the needle on which it is ſuſpended as hard and finely pointed as the weight to be hung on it will allow, and the needle-cap properly made with a fine apex and poliſhed; for beſide the error of adjuſtment, which may while it is uſing be detected, and either remedied, or the obſervation made truly, even when that error remains, (by taking the mean of the greateſt and leaſt altitudes the level affords in a ſemi-revolution of the mirror round its axis, as has been obſerved before), it is liable to no other error but from a pendulous motion of the weight or a tremulous motion of the inſtrument itſelf from its being agitated by the wind; and if it is affected by either of theſe, it will be viſible by the vibration or tremor of the image in the level compared with that in the quadrant which remains motionleſs. And I have found that it reſiſts the action of the wind ſo well as to admit obſervations to be made in almoſt any weather in which a perſon would chuſe to take them in the open air.

But I have obſerved its ſteadineſs to depend on the following circumſtances:

1ſt. That the whole inſtrument be as light as is conſiſtent with the neceſſary ſtrength, eſpecially the parts remote from the point of its ſuſpenſion, as the edges of the mirror and the lower part of the frame and ſtem; on which account the mirror ſhould be a thin plate of glaſs, and cannot be more than three inches in

in diameter; but it would be much better if it did not exceed two and a half; and alſo if it were made, like a plano-convex lens, thin at the edges: indeed the mirror need not be ſo broad as the ſurface of any fluid ought to be for this purpoſe, becauſe to the very edges it is a plane, which is not the caſe with any fluid; and the contact of the limbs of the images of the ſun and moon may, when their altitude is little more than one degree, be diſtinguiſhed in a mirror of no greater diameter than two and a half inches. I have experienced that it is eaſy to find the reflected image in a mirror of leſs than two inches diameter, by advancing the eye with the quadrant as cloſe to it as poſſible; and any perſon uſed to make obſervations with a quadrant and to hold it ſteady will find this not difficult.

The action of the wind is exerted more againſt the mirror, as being a broad continued ſurface, and immediately expoſed to it, than againſt its frame underneath, which is alſo ſcreened from it by the ſides of the veſſel; the top of which, when it is wider than the mirror, ſhould perhaps be cloſed with ſuch a moveable cover, as would ſcreen every part of it except that occupied by the mirror, and prevent the wind from blowing down into it. Whatever perturbating force is exerted againſt the mirror is at the ſame time imparted to the frame, and the momentum of both produces a vibrating motion laſting ſome time, and occaſions pendulous vibrations of the weight, which are of longer duration

than the tremors of the inſtrument itſelf, and are more to be prevented.

2dly. The greater the weight is, the more will both be ſuppreſſed; ſo that the rectification of the inſtrument is within one's power; it ſhould not be much leſs than two pounds: it ſeemed to me that the greater the tremors were, the leſs were the vibrations and vice verſa; but the former were, as might be expected, diminiſhed, and the latter increaſed, by ſhortening the ſtring *t*, or by lengthening the ſtem, and contrarywiſe.* In calm weather this level will, when the arm *K* ſupporting

* I have tried different lengths of the ſtring and quantities of the weight, without being able to make out a theory of their effects, which yet is the more important, as it is part of the theory of the pendulum; for the level with its weight is a compound pendulum, conſiſting of two maſſes of matter connected by a ſtring or wire: when I ſuſpected that the claſticity of the ſtring might ſuffer it to extend, and ſo to permit the tremulous agitations of the mirror and ſtem without propagating motion to the weight, and exciting its whole reſiſtance, I ſubſtituted a ſtrong wire for the ſtring (each two feet long) but found that the tremors of the mirror were more laſting, and the vibrations of the weight ſooner excited by the wire than the ſtring; ſo that the latter better anſwered the deſign of the inſtrument: its length ſeemed of the leſs conſequence, ſince any length tending to ſuppreſs the tremors encreaſed the vibrations and *vice verſa*: I therefore contracted the ſtring to the length of an inch.

The tremors of the ſtem were much quicker than its oſcillations from its own gravity would be, and far more ſo than thoſe of the weight could be ſuppoſed; theſe tremors therefore were like the vibrations of a muſical chord, ſaving that this is ſtretched beyond its natural length, except when in the middle point of each vibration; whereas it is more likely that during the tremors of the mirror, the weight

ſupporting it, is ſcrewed to a tree &c. be made ſteady enough by a weight, though it be not ſuſpended in water; and ſince a

 ſtone

weight ſhould be kept raiſed in the water, and that the interval between the point of ſuſpenſion and the weight ſhould be thus ſhortened, than that the ſtrong wire with which I connected it to the ſtem, ſhould be ſtretched: hence ſince ſuch vibrations of muſical ſtrings are greateſt juſt after the impulſe which produces them, and die away quickly, perhaps if ſuch a compound pendulum were applied to a clock, and the ſtroke of the pallet applied near the end of its aſcent, it would be likely to acquire the cycloidal properties, by being ſhortened towards the extremities of its arch, during moſt of the inſtants of time it ſpends there; eſpecially ſince the ſame force or maintaining power, would enlarge or diminiſh the muſical and pendulous vibrations together, and might therefore adjuſt the length of the pendulum in every part of its vibration, ſo as to make its accelerating force as its diſtance from the point of reſt.

As the extent of the diſadvantages which would attend an enlargement of the ſize or weight of the inſtrument beyond what is neceſſary, may not be obvious, I muſt obſerve, that the accuracy of obſervations made by it chiefly depends on its ſteadineſs in reſiſting the wind; and ſince the action of the wind is not momentary but continued, its effect will be probably proportional to the extent of ſurface expoſed to it, and alſo to the quantity of matter it impels, and will be in a ratio compounded of both theſe, i. e. of the ſurface of the mirror and frame which is in the duplicate ratio of the diameters or dimenſions of the ſeveral parts; and of the ſolid content of the inſtrument, which is in the triplicate ratio of the ſame diameters: ſo that if the diameter of each part be made twice greater in one of theſe inſtruments than in another, the action of the wind on the former will be exerted on four times a greater ſurface than is expoſed to it by the latter, and on eight times a greater maſs: it will therefore elevate the extremity of the ſtem, and with it the appended weight, in each tremulous vibration, four times higher, in the larger than in the ſmaller inſtrument: and ſince the momentum of each is proportional to the verſed ſine of the arch of its vibration (which is the height to which the weight is raiſed), and alſo to its quantity of matter; and the magnitude and duration conjointly of the tremors are as the momentum; ſo the diſadvantage

ſtone or any thing almoſt will ſerve as an occaſional weight, as alſo any veſſel to hold water, in which caſe nothing is wanting to it that may not be every where ſupplied; and as the inſtrument, with the hook or arm *K* which ſupports it, might be made of a ſize not much greater than in fig. 2. ſo it may, when conſtructed in this form and ſize, be, as a concomitant to a ſmall Hadley's ſextant, an uſeful acquiſition to a traveller who would equip himſelf expeditely: with this view, and for ſecurity againſt accidents, the mirror and the whole frame might be made of poliſhed ſteel: and when it is conſtructed upon this ſmaller ſcale, the ſtem may with advantage be made a little longer than in proportion to the figure, becauſe its length has relation not ſo much to the dimenſions of the other parts, as to the length of the ſtring, and quantity of the weight appended; both which may remain unaltered.

I WOULD not have propoſed the inſtrument here deſcribed, if I imagined that the level mentioned by Mr. Vince (in his Practical Aſtronomy, page 33), or any other one, were equally eaſy in its conſtruction and uſe, and capable like this of being every where fabricated: that deſcribed by him is (as I underſtand

of the inſtrument of double ſize will be to that of the other one in a ratio compounded of both, i. e. of 4×8, (or of 32) to 1, which will be reduced to the ratio of 16 to 1; becauſe when the length of the ſtem is doubled, the power of the weight to ſuppreſs the tremors is doubled: from whence appears the neceſſity of making it as ſmall and light as poſſible, becauſe the appended weight cannot be conveniently enlarged.

underſtand it) a plano-concave lens, whoſe plane ſide is uppermoſt and a reflecting level; covering a box filled with ſpirit of wine, except a bubble, which reſts under the higheſt part of the concave ſurface; being a ſort of ſpirit-level; but to procure it the accuracy of ſuch, the radius of its concavity ſhould be as great as that of the ſpirit-level; and I ſuppoſe it muſt be difficult to grind it truly to ſo great a radius *; and to find that point in the concave

* If any kind of ſpirit-level be made uſe of to fix a reflecting plane horizontal, and its bubble were, from the attraction of coheſion between the glaſs and the included liquor, to reſt below the ſummit of the concave arch of the tube, &c. the error this will produce will be of an angle, double that meaſured by the arch of the tube between the point of reſt of the bubble and the ſummit of the tube, (as can be collected from what has and will be mentioned); now I have intimated above, that not only the ſenſibility but the accuracy of the ſpirit-level, or its exemption from errors ariſing from this cauſe, will be improved by enlarging the radius of curvature of its tube: and becauſe it has been aſſerted, that "it could not be depended upon "to greater accuracy than one minute; for that when the radius of curvature of "the level is very great, the force with which the bubble riſes to the higheſt point "is ſo very ſmall, that the leaſt adheſion of the liquor to the glaſs will ſtop it, and "its place of reſt will be uncertain,"* which ſeems to intimate, that enlarging the radius of curvature would be of little advantage: I hope I ſhall be pardoned for inſerting the following ſtatement, which inclines me to think otherwiſe; though an acquieſcence in this as a fact would be a recommendation of the level here deſcribed, which is not liable to ſuch objection, and though it is only with diffidence and reluctance I can diſſent from the very accurate and ingenious perſon who aſſerted this, and whoſe poſition I would not venture to controvert, had he ſtated his having aſcertained the fact by experiment.

Let there be two ſuch levels, whoſe curvature is repreſented by the arches AF, AG, fig. 7, AC being the radius of the former, and the point A the ſummit of both;

* Vide Ludlam's Remarks on his Aſtronomical Obſervations.

concave arch, at which a tangent plane would be parallel to the upper ſurface, and which point muſt be the centre of the bubble: alſo

both; and let the centre of the bubble reſt, not at the point A but at D, making the angle ACD the error of the inſtrument, whoſe ſine is BD, and verſed ſine BA: the ſame line BA will be the verſed ſine of the angle of aberration in all levels of ſimilar conſtruction and regular ſhape, whatever be the degree of curvature of their tubes: for becauſe the force with which fluids move in any direction through a pipe or orifice is q. p. equal to the weight of the ſuperincumbent fluid, which in this caſe is that of a column of liquor, whoſe altitude will be AB (if while the centre of the bubble is at D, its extremity extends to A; but leſs, and equally ſo in both tubes, if the extremity reſts at a diſtance from A on either ſide); ſo the weight and height of this column muſt be in both levels the ſame, becauſe it is counterpoiſed by the attraction of coheſion (I ſpeak not of electric attraction) arreſting the liquor, and preventing the extremities of the bubble from ſettling in an horizontal plane; which attraction is the ſame and invariable in tubes of equal diameters whatever be their curvature, and therefore in both tubes AF, AG, the centre of the bubble will reſt in the line BD: let it reſt in the tube of leſſer curvature or greater radius at the point E, and in the other tube AD in D; then the arch AE, meaſuring the angle of aberration in the former tube will be to the arch AD the aberration in the latter in the inverſe ratio of the line BE to the line BD: theſe lines are the ſines of the angles; but the ſine or ordinate of a circle is a mean proportional between the verſed ſine or abſciſſe and its complement to the diameter; for if lines be drawn from the extremes of the diameter to any point in the circumference, as for inſtance, to D in the circle AD, they conſtitute an angle in a ſemicircle, which muſt be a right one, (El. 3. 31.), from whoſe vertex the ſine DB falls perpendicular on the baſe; ſo that the verſed ſine AB is to the ſine BD as this BD is to the complement of the diameter BH (El. 6. 8.); i. e. the rectangle AB, BH is equal to the ſquare of BD (El. 6. 16.); alſo in the circle AE the rectangle under the ſame abſciſſe AB, and the complement to its diameter is equal to the ſquare of BE: but theſe two rectangles, ſince one of their ſides AB is common, muſt be in the ratio of the other ſides; i. e (becauſe AB is ſo very ſmall that it may be neglected), in the ratio of the whole diameters, or of the radii which are as the diameters; which ratio is that of the ſquare of BD to the ſquare of BE; conſequently when the ſines are as 1 to 2, the diameters or radii

alſo were it properly conſtructed, it would require to be adjuſted every time it is uſed or ſtirred from its place, unleſs it were ſet upon another level plane, and to make this adjuſtment would require the light of the day or of a candle; ſo that it would not anſwer to make by night and in any place obſervations of the ſtars, which are often the only ones a traveller has an opportunity of making. I apprehend it would require at every obſervation a conſiderable part of the trouble of adjuſtment, which in the level here deſcribed ſerves once for all; for after being adjuſted, it requires no farther care than to keep the needle duly pointed.

As an inſtrument which does not, like this one, detect its own errors, may gain credit for an accuracy which it has not, it will be unfair to judge of the preſent level only from a compariſon of it with ſuch a one: the faireſt trial of its adjuſtment, &c. will be made by obſervations taken in calm weather from the ſurface of a fluid of ſufficient extent, and not covered by glaſs planes. If the weight were to be faſtened to the ſtem without the intervention of a cord or hook, the tremors of the mirror would be ſuppreſſed, and the wind would communicate no motion

radii will be as 1 to 4: if the ſines were as the radii, or as 1 to 4, the angles or arches AD, AE would be equal, but ſince BD is by ſuppoſition only half BE, and the angles are ſmall, the arch of aberration AE will be little more than half the arch AD: and hence it appears that the error of the level will be q. p. in nearly the inverſe ſubduplicate ratio of the radius of its curvature; ſo that when the radius is as 1, 4, 16, &c. the aberration will be as 4, 2, 1, &c. or as 1, $\frac{1}{2}$, $\frac{1}{4}$, &c.

motion to it, except a pendulous vibration affecting the whole maſs. The Reverend Mr. Auſtin, for whoſe judgment in theſe things I had due reſpect, adviſed me to contrive the machine on this principle; and accordingly I made it in the following faſhion, which, all circumſtances conſidered, I found to afford the moſt commodious general uſe of it, and to render it ſufficiently portable, as the whole may be contained in a round braſs caſe nine inches in length, and leſs than three in diameter, ſuppoſing that of the mirror to be two inches and a half, nor need it be much heavier than the glaſs planes commonly uſed, together with the quickſilver, &c. required with them, even when made of a ſize as large as here propoſed. It is repreſented fig. 8. in a vertical ſection through the middle and one third of the intended ſize; though I think, if it were made of only twice the dimenſions in the figure it might be ſufficient.

MN is a cylindrical veſſel or caſe, made of braſs or wood, which contains the level: in the middle of the bottom of it, (which ſhould not be liable to bend) is firmly ſcrewed a ſtrong braſs rod *EE*, tapering upward; and in the upper end of this is inſerted the needle *L*, on whoſe point the mirror reſts: the plate *A* is the mirror, *B* the braſs plate cemented to it, in which is fixed the conical cap that receives the needle: this plate *B* is immoveably ſcrewed and ſoldered to the end of the pipe *CC*, which pipe is ſeven inches and a half long, and widens downward, having a hole through its ſide juſt under the projecting part at *g*, to prevent the condenſation of air within it, when it is immerſed in

in water, &c. to that depth: this pipe, towards its upper end, is ſo narrow, as only to allow a ſmall vibration of it on the point of ſuſpenſion *L*, without bringing its ſides into contact with the upright ſpindle *EE*, for thus it will preſent a leſs ſurface to the impulſe of the wind; but when it vibrates on the needle, the loweſt part only of the pipe muſt be allowed to ſtrike againſt the rod *EE*, leſt the point of the needle ſhould be broken. The weight is repreſented at *PP*; it is a maſſive ring of braſs weighing about one pound and a half, ſoldered on the pipe *CC*, (or it might be caſt in the ſame maſs with it); after which the whole was carefully turned in a lathe, in order that its ſides might be equally thick all around; for thus they will lie equidiſtant from the rod *EE*, when this is in a vertical poſition, and the inſtrument hangs freely on the point of ſuſpenſion; i. e. when the caſe *MN* is ſet on its baſe, and reſts ſteady on the ground or ſome level ſtand, or is ſo ſupported, that it remains erect, and in circumſtances fit for making an obſervation. To render it capable of being carried about in its caſe without injury, a braſs ring, having the form and indenture in it repreſented at *FF*, is faſtened within the caſe *MN*; which is done by four ſcrews paſſed though its ſides, that it, and alſo the level, may be introduced and taken out; and the upper part of the weight *PP* has a ſhoulder formed on it to fit and fill the indenture *b* in the ring, into which it is unerringly introduced by the conical part of the weight above the ſhoulder, when the weight is puſhed up to it; and this is done by advancing againſt it the ſcrews *DD*, which move through the bottom plate of the caſe

MN, and are furniſhed with locking nuts *ff* on the outſide, to prevent their looſening: when the inſtrument is thus puſhed upward in the caſe, the needle-cap at *L* is raiſed above the needle, and cannot injure its point, but may be let down upon it again by withdrawing the ſcrews *DD*. To ſteady the weight and mirror, any convenient liquor, as water, vinous ſpirit, &c. is poured into the caſe through the open orifice *O* in its ſide, to the height of the ring *FF*, and may be let out again by another orifice, (occaſionally ſtopped), near the bottom; to prevent which from running out at the ſcrews *DD*, plates of leather are interpoſed between the nuts *ff* and the bottom of the caſe: and if a ring of leather were likewiſe faſtened on the ſhoulder *g* of the weight *PP*, or in the indenture *h* of the ring *FF*, and freſh linſeed oil, eſſential oil, or any other, which would not ruſt the needle, were ſubſtituted inſtead of water (if by experience oil ſhould be found admiſſible) it would, by the preſſure of the ſhoulder of the weight *PP*, when ſcrewed up againſt the leather at *h*, be always kept in the bottom of the veſſel from running out, and nothing would be wanting to fit the inſtrument at all times for obſervation but to take off the cover *HH*, to withdraw the ſcrews *DD*, (keeping the nuts *ff* in their places, and holding the caſe upright that the oil may remain at the bottom), and then to ſet down the inſtrument. But the ſame oil could not be long uſed; for unleſs every part of the metal which it could touch were coated with tin, it would grow foul and viſcid by diſſolving the braſs, as it would alſo of itſelf in time; and in cold weather and climates

climates it would by freezing or growing viſcid be rendered utterly unfit for this purpoſe: it has been diſapproved of by a perſon of better judgment than mine, and is only propoſed for experiment, ſhould the inſtrument be uſed in viſiting hot climates, where the needle would ruſt, and other liquors be ſcarce.

The want of adjuſtment of the level in this conſtruction is diſcovered by the motion of the image reflected from it as before-mentioned; and it is corrected by noting that diameter of the mirror, in which, during its revolution, the image appears moſt and leaſt elevated; then ſuppoſing a vertical plane in that diameter to divide the whole inſtrument, marking a line acroſs its baſe, i. e. acroſs the bottom of the weight; if a due portion of the metal of the weight near that line be filed away on the ſide oppoſite to that in which the image is ſeen higheſt or loweſt, the mirror will ſettle to the juſt horizontal poſition. For inſtance, ſuppoſe the moſt elevated place of the image ſeen in the mirror to be at the point *a* in the diameter *a b*; and that a perpendicular plane paſſing through the ſame interſects the baſe or bottom of the weight in the line *c d*; if ſome of the metal be raſped away near *d* on the ſide oppoſite to *a*, then the ſide *c* of the weight will be heavier than the ſide *d*, and will deſcend and approach nearer to the rod *E E*; and the ſide *a* of the mirror which was too much elevated, will at the ſame time ſubſide to a level poſition: and when this is once effected exactly, ſeeing that the inſtrument is one ſolid inflexible maſs, whoſe centre of gravity cannot alter,

it is plain that unleſs it ſhould receive ſome injury, it will never after require any adjuſtment; but if it ſhould, it may eaſily be rectified in the ſame manner.

To make the level more ſteady in the time of obſervation, a tripod ſtand like that of a teleſcope might be fixed to the bottom of the caſe *MN* to ſupport it; the feet of which might be contrived to turn upward and reſt againſt the ſides of the caſe, when the inſtrument is not uſed. I could deſcribe a mode of doing this, but can add no more to this paper, the length of which demands from me an earneſt apology; obſerving only that as this level, if made on either of the two conſtructions here mentioned, may be adjuſted to the utmoſt accuracy, ſo it alſo reſiſts the agitation of the wind as well as the fluid level covered with glaſs planes, unleſs the quickſilver in the latter be likewiſe ſheltered by a glaſs plate laid on its ſurface, which if the quickſilver ſhould happen to be ſpilled would be uſeleſs; and ſuppoſing the plate to be of ſuch uniform thickneſs and denſity, and to float ſo equidiſtant from the depreſſed margin of the quickſilver on every ſide, no air bubbles likewiſe intervening nor accidental contact with the ſides of the veſſel, that it would naturally lie parallel to the ſurface of the fluid, yet if this ſhould accidentally be ſo overſpread with duſt or foulneſs, as to raiſe one edge of the plate, and depreſs the other, even ſo little as the thirty-three-thouſandth part of an inch, it might give an altitude one minute of

of a degree erroneous:* and as theſe accidents are likely to happen to a perſon uſing that kind of level in travelling, ſo I am perſuaded the inſtrument here deſcribed would remain longer ſerviceable to a traveller, than any other for this purpoſe that I am acquainted with: and it is as being the moſt ſafely portable of any, and alſo manageable abroad at night when others are not ſo, that it is chiefly recommended; though after many comparative trials of the fluid level with this one, I would, without conſciouſneſs of partiality, generally prefer the latter; for it is in fact a plummet, and may be made and adjuſted to have

* If the radius of a circle be ten inches, each inch divided into a thouſand equal parts, i. e. in the whole ten thouſand, the ſine of the arch of one minute is (not quite) three of thoſe parts, or the $\frac{1}{3333}$ part of an inch: if the radius be only the $\frac{1}{5}$ of this, or two inches, the ſine of one minute will be only the $\frac{1}{5}$ of $\frac{1}{3333}$ of an inch; i. e. the $\frac{1}{16665}$ part: conſequently if the reflecting plate or mirror above-mentioned were four inches in diameter, and it were accidentally tilted up at one edge, and depreſſed on the oppoſite one, even ſo little as the $\frac{1}{16665}$ part of an inch, it would be one minute of a degree oblique to the horizon: but I have ſhewn, that the greateſt error of obſervation by a reflecting level, is always double the error of the ſpeculum; hence half the above angle of obliquity of the ſpeculum to the horizon, or an elevation and depreſſion of its oppoſite edges from it, of the $\frac{1}{33330}$ part of an inch, (and how ſmall a quantity this is, I leave to the conception of the reader) will, if the diameter of greateſt obliquity happen to be in the plane of viſion, produce an error in the obſervation amounting to one minute: the ſame is true of the level here deſcribed; ſo that the property of a ſcrew in producing ſmall motions is admirable, ſince its adjuſting ſcrews, even at the ſame diſtance (two inches) from the centre of motion, can in rectifying the ſpeculum, be made to advance with preciſion enough, through a ſpace far leſs than this, even ſo ſmall as the one-hundred-thouſandth part of an inch.

have the accuracy which in aſtronomical uſe, and like circumſtances, the plummet affords.

It is eaſy to conceive that the greater the ſize of this level is made *in length*, when it is conſtructed in the form in fig. 8, the more accurate will it be, and ſteady in reſiſting the agitation of the wind. Alſo that this faſhion of it admits the more eaſy adjuſtment, as the mirror can be turned quite round on its axis, and the higheſt and loweſt place of the image inſtantly diſcovered, conſequently its error, if it has any.

On the NATURALIZATION *of* PLANTS. *By* JOHN TEMPLETON, *A L.S. communicated by the* BISHOP *of* CLONFERT.

Read, Nov. 23, 1799.

THE naturalization of plants is an object of such importance, and a subject that is at present so little understood, that any attempt to extend our knowledge of it, however trifling, may still tend towards improvement, and perhaps serve as a foundation, on which at some future period a more perfect structure may be erected. Many experiments are yet wanting, much remains to be yet done, and, like other branches of knowledge, it will require the united efforts of numbers to bring it to perfection.

THE same Almighty hand that formed the earth, has scattered in far distant regions vegetables which the necessity or luxury of man excites him to endeavour to accumulate about his home. And if we at the present time survey the different nations of the earth, we will find that most of them have received great and important benefits by the introduction of foreign plants; and that there is no country, however numerous its collection of plants, but may yet receive considerable advantages by the naturalization of others.

BOTANY,

Botany, a ſcience which every one engaged in the ſtudy of will readily acknowledge to afford one of the pureſt of human pleaſures, from the introduction of exotics derives its principal ſupport; and certainly whatever tends to facilitate this amiable ſtudy is truly deſerving of the attention of every philanthropic mind. As all botaniſts cannot have an opportunity of examining plants in their native ſoils at proper ſeaſons, it is therefore only by tranſplanting and cultivating, they can become acquainted with the productions of diſtant countries; and to cultivate them with ſucceſs we muſt derive our information principally from the plants themſelves: each has certainly a peculiar character, which were we truly acquainted with, thoſe tedious experiments with each newly acquired ſpecies, which now nearly exhauſt the patience of all lovers of plants, would be no longer neceſſary; gardening might then boaſt of being eſtabliſhed on ſcientific principles, and would then never adopt rules contrary to what nature dictates.

> Nor will ſhe, ſcorning truth and taſte, devote
> To ſtrange and alien ſoils, her ſeedling ſtems;
> Fix the dark ſallow on the mountain's brow,
> Or to the moſs-grown margin of the lake
> Bid the dry pine deſcend. From nature's laws
> She draws her own: nature and ſhe are one.
>
> Mason's English Garden, B. III, line 226.

By

By our preſent imperfect knowledge of the phyſiology of plants, we are neceſſitated to accept of every aſſiſtance within our reach: and plants being ſo immediately connected with every modification of the atmoſphere, meteorology, which has hitherto been conſidered as an object of curioſity, is a ſource from which we may derive much uſeful information.

Heat being found to increaſe or decreaſe nearly in a regular progreſſion, according to the degrees of latitude, if the latitude of the place where a plant is found be known, by conſulting Mr. Kirwan's Table of the Mean Annual Temperature of different Latitudes*, we may find whether the temperature nearly correſponds with our own. Or, ſuppoſing the mean annual temperature of Dublin, lat. 52°, equal 50 of temperature, by adding one for every degree of latitude ſouthward, and ſubtracting one for every degree of latitude northward, we have the temperature correctly enough for our purpoſe. For theſe calculations need not be carried to the greateſt degree of correctneſs, as we know that, if we except a few, plants have a conſiderable range of latitude; thoſe which cannot bear froſt, being found to extend from the northern to the ſouthern verge of the torrid zone, and many of thoſe which grow on the ſouthern limits of the temperate, to approach the borders of the frozen zone. Thus of the Lapland

* See an Eſtimate of the Temperature of Different Latitudes by R. Kirwan, Eſq; page 17.

plants near three hundred are found in the environs of Paris*, many of them much farther ſouth, and ſome, as the Water Lillies (Nymphæa), Sundew (Droſera), Arrow-head (Sagittaria), &c. even natives of India †.

In the latitude 44° on the European, and 34° on the American continent ‡, it is not unuſual for water to be frozen in January; and as ſome ſeaſons are much more ſevere than others, plants growing conſiderably farther to the ſouthward would be liable to ſuffer by cold in ſuch ſeaſons, if nature had not provided a remedy by their manner of growing, which enables them to reſiſt the cold of ſuch rigorous ſeaſons; and on this account many of them will be found to thrive, when tranſplanted nine or ten degrees further north than their native ſtations.

From antient authors it appears that Italy formerly experienced the ſame degree of cold as the American continent under the ſame parallel of latitude does at preſent. Therefore it is highly probable, that Italian plants not introduced into Italy ſince that time, might in a ſeries of years be changed from their now tender to their once hardy ſtate.

But

* See Flora Lapponica & Thuillier's Flore des Environs de Paris.

† See Hunters Evlyns Sylva, p. 552.

‡ See Kirwan on the Temperature of Different Latitudes, p. 50.

But in conducting ſuch an experiment as the naturalization of plants from a ſouthern to a northern climate, ſo many minute circumſtances require our attention, that few people have either time or patience to reach the wiſhed for goal.

But a careful attention to the characters which the plants themſelves preſent, will enable us to proceed with more certainty, and hopes of having our endeavours crowned with ſucceſs.

By the appearance of the roots and leaves we may nearly determine in what kind of ſoil the plant is moſt likely to thrive. Robuſt roots and fleſhy or rigid leaves require a dry ſoil, according to their thickneſs; ſtiff clay or ſandy loam, as beans, peach, and apple trees; robuſt ſpongy roots which have a tendency to mat near the ſurface with thin leaves, as the Alder (Betula Alnus), Willows (Salix), require a ſomewhat ſtiff ſoil with moiſture; many of the Salix genus will not grow with their accuſtomed vigour, in a light turfy or peat mold ſoil, for want of the neceſſary reſiſtance to the roots, although ſuitable in reſpect to moiſture. Slender, hard and wiry roots, as thoſe of the pine, Ciſtus, &c. require dry, ſandy, or gravelly ſoils. And extremely fine and hairlike roots, as thoſe of Erica, Halmia, Rhododendron, &c. muſt have a ſoil whoſe particles will not impede the ſhooting of their tender fibres, and with a ſmall but regular degree of moiſture, that the roots, which by their form cannot reſiſt the ſlighteſt drought, may not be deſtroyed. Plants in a warm climate

perſpire more than in a cold one; ſo in a warm they require much, and in a cold one little moiſture. Therefore, when tranſplanted from a warm to a cold climate, they ſhould have a dryer ſoil, and from a colder to a warmer, a moiſter one, than their native ſtation.

In the firſt caſe, not being able to perſpire the ſuperabundant moiſture, they will be rotted; and in the laſt, not having moiſture ſufficient to ſupply the loſs by perſpiration, the growth will be ſlow, diſeaſe and death will follow, unleſs they receive a timely ſupply of moiſture: by the red or yellow colour of the leaves we may diſcern the approach of the firſt evil, and by the ſtunted growth, and ſmall curled leaves, that of the laſt. A large quantity of pure circulating fluid ſeldom injures plants, but ſtagnant water is certain deſtruction to almoſt every vegetable.

After having determined the moſt ſuitable ſoil, we muſt afterwards ſtrive to give each plant a proper ſituation. It is well known, that plants from a ſhady will not thrive well in an open, nor plants from an open in a ſhady ſituation. But the neceſſity of a natural ſituation is by no plant more evidently illuſtrated than by the common myrtle (Myrtus Communis). Even at Glenarm, in the latitude 54° 56′ N. it grows with great luxuriance contiguous to the ſea, and braves our coldeſt winters; yet all attempts to naturalize it in an inland ſituation, ſeveral degrees

degrees farther ſouth, and in a much more genial climate, have hitherto proved unſucceſsful. The olive tree cannot poſſibly be cultivated in the interior of Aſia or America, though the latitude be in other reſpects favourable, nor is it fruitful when excluded from the ſea breezes *.

The cauſe of this may be that near the ſea the temperature is more regular than within land, and ſudden changes are perhaps unfavourable to evergreens: for we always find thoſe with broad leaves grow beſt in the ſhade, and thoſe with narrow leaves on elevated places, in both of which ſituations the temperature is more regular than in open expoſures or confined vallies. And we may often obſerve plants growing on a ſomewhat elevated ſituation, if ſheltered from ſtrong winds, leſs hurt in a ſevere winter, than others in low warm and ſheltered places. For vapour being raiſed in ſuch places during the day, produces a greater degree of cold by condenſation and evaporation in the night, than is experienced in other places where the coldneſs of the air prevented the riſe of vapour during the day. From every obſervation it appears, that thoſe plants which have the leaſt ſap in winter, or whoſe ſap is of a reſinous or oily nature, ſuffer leaſt from cold, and that the principal cauſe of deſtruction is the veſſels being burſt by the freezing of the ſap. The hoar-froſt, which is always moſt abundant in vales, tends in a great degree to promote this;

* See Saint Pierre's Studies of Nature, tranſlated by Hunter, Vol. I. page 60. Dublin Edition.

this; for being changed into water, part only of this water is evaporated during the day, the reſt remains to be converted into ice by the cold of the enſuing night. This icy covering encreaſes the cold, till the vital principle*, and reſiſtance given by the formation of the bark to the entrance of cold, are overcome, the ſap frozen, and at the ſame time the veſſels burſt by the expanſive force of freezing. This gives the reaſon why plants in a ſituation where the ſun does not ſhine on them to thaw the hoar-froſt, ſuffer leaſt in ſevere ſeaſons; and that plants removed in Autumn, unleſs the ſhoots are completely hardened, will be more liable to be injured by froſt than thoſe of the ſame ſpecies, the deſcent or fixation of whoſe ſap has received no check by tranſplanting. Miller remarks, that thoſe plants which were removed in the Autumn of 1739 were moſtly killed by the cold of the enſuing winter, while many of the ſame ſpecies eſcaped uninjured: and the ſame may be always obſerved after every ſevere winter.

Few deciduous ſhrubs agree with ſhade: their natural place is the ſunny outſkirts of the foreſt; and when otherwiſe ſituated, long

* See Smith's Tracts relating to Natural Hiſtory, page 177, and Philoſophical Tranſactions for 1788.

† The following experiments may throw ſome light upon the cauſe of plants remaining unfrozen, when the ſurrounding water is frozen. Water encloſed in ſealed glaſs globules remains unfrozen, 'till the thermometer deſcends to twenty-four; unſealed ones freeze and burſt immediately on being cooled down to freezing water. Oil encloſed in the ſame kind of globules continued unexpanded, and conſequently the globules unbroken, when placed in a mixture of ſnow and ſal ammoniac, and cooled below 0.

long and ſlender branches, with large thin leaves, ſhew their unhealthy ſtate. From theſe the climbing plants are eaſily diſtinguiſhed by their tendency to contortion, or ſhooting forth roots or tendrils. To the deciduous climbers a ſlight ſhade is not hurtful, as it is only there they can find the neceſſary ſupport; but in the deep receſſes of the foreſt, the evergreen climbers will ſpread around their tangling branches, and thrive with wild luxuriance,

> Where ſcarce a ſunbeam wanders through the gloom.
>
> THOMSON'S SEASONS.

never appearing, if the ſoil is ſufficiently moiſt in ſummer, to be hurt by the thickeſt ſhade of deciduous trees.

THE ſhade is alſo the natural ſituation for young plants. By the parental ſhade they are protected from the drought of Summer, and the cold of winter. The more a plant is ſhaded in winter, and the nearer it is to any large body, the leſs danger it will be in of ſuffering from froſt. For when a plant or water is ſo ſituated as to be overtopped by trees, a great part of the hoar or froſty particles, which would fall on it, is intercepted. Under trees we may often obſerve water unfrozen, and plants unhurt by the ſeverity of cold, and many retaining their leaves; when water at a ſmall diſtance is frozen, and plants of the ſame ſpecies, but unſhaded, loſe their leaves and ſuffer conſiderably. As large bodies are not eaſily cooled, the cold is in ſome degree mitigated by the

ſtems

ſtems of large trees. That this is the caſe may be perceived, the twigs and ſmaller branches being covered with hoarfroſt, when the trunk and larger branches remain uncovered.

Dr. Wilson of Glaſgow obſerved, that when a great degree of cold prevailed, paliſades extending outward from a houſe, and alſo from a large pillar, were covered with hoarfroſt, in the moſt regular manner, according to their proximity or diſtance from the houſe or pillar, thoſe next the houſe or pillar remaining free from hoarfroſt, while the more diſtant ones were entirely covered *. This accounts for the fig-tree ſhoots, mentioned by Miller, being killed when growing out from the wall, at the ſame time when the other ſhoots cloſe nailed to it eſcaped unhurt †. From this circumſtance, moſt people have affixed thoſe plants which they wiſh to naturalize to the climate againſt walls. But when put to a wall, care ſhould be taken that they are ſheltered from ſtrong winds, which generally injure the leaves and young ſhoots, thereby deſtroying the plant if it is not vigorous. On this account the ſtems of large trees are preferable for climbing plants, and there they muſt always be more admired as appearing more natural.

But inſtead of affixing to walls thoſe plants which require no ſupport, we might cultivate them in pots or boxes, which may be

* Philoſophical Tranſactions, Vol. lxx, p. 471, 2.

† Miller's Dictionary, Article Ficus.

be placed in their proper expoſure during the ſummer, and, until their hardineſs is determined, removed under the fir or other trees in winter, the thickneſs of whoſe ſhade ought to be proportioned to the apparent tenderneſs of the plant.

These pots or boxes ſhould be always ſunk in the earth, and in winter the ſurface covered with moſs. The drier the ground the better, for ſunken pots are liable to be too damp.

The beſt manner of treating Ericas is to place them in a proper ſituation in the ſpring, and on the approach of ſevere weather to fix branches of ſpruce-fir about them, augmenting the covering as the cold encreaſes. But as the cold ſeldom becomes ſuddenly ſevere, and a ſlight froſt does them little injury, the Erica Tubliflora, one of the moſt tender, bearing about 29° of Fahrenheit's thermometer; it is beſt not to begin covering too ſoon, leſt, as they are plants that require a very ſmall degree of heat, they ſhould be made to ſhoot, in which caſe the ſlighteſt froſt will perhaps deſtroy them. To protect herbaceous plants from froſt, moſs is the moſt proper covering, by remaining alive through the winter; even after being pulled up, it is not liable to heating and putrefaction, as all dead vegetable ſubſtances are, by which they impart to the plant heat and moiſture (the two principal agents which cauſe vegetation) thereby putting the vegetating powers in action, and filling the plant with ſap, at an improper ſeaſon. In our culture of annuals our only care is di-

rected to placing them in proper ſoils and expoſures. For, that no region of the earth ſhould remain uninhabited, with a liberal hand have the annual plants been diſtributed; from theſe do men and animals derive their principal ſupport, and of all the vegetable kingdom, they are beſt adapted for naturalization. By bringing their ſeed to perfection in a ſingle ſeaſon, they are capable of cultivation in a greater variety of climates than any other vegetables. And the ſeed, being equally undeſtroyed by natural heat and cold, lies dormant, till genial weather calls forth its latent powers, and urges it to vegetation, whether among the frozen ſnows of Siberia, or the burning ſands of Africa. The Reſeda odorata (Mignionette), a native of Egypt, and Helianthus annuus (Sunflower), of Mexico and Peru, ripen their ſeed, and are thereby perpetuated in our northern latitudes. St. Pierre * ſays, the peaſants of Finland cultivate tobacco (Nicotiana Tabacum), with ſucceſs, beyond the ſixty-firſt degree of latitude; and that barley ſucceeds in the very boſom of the North. Amidſt the rocks of Finland he ſaw crops of this grain as beautiful as ever the plains of Paleſtine produced †.

When we endeavour to naturalize plants, that we may diſtinguiſh thoſe which offer the faireſt proſpect of ſucceſs, a compariſon of the exotics with the natives of the ſoil will be our ſureſt

* See St. Pierre's Studies of Nature, tranſlated by Hunter, Dublin Edition, Vol. I, page 604.

† See ſame Work, page 667.

ſureſt guide. Thus we find, that throughout the froſty regions of the north, the trees, ſhrubs, bulbous and perrennials, complete their ſhoots, and, before the cold of the winter commences, encloſe in hybernacule or ſcaly buds, the embryo for the coming year. And there is every reaſon to believe that all exotics which ceaſe growing, and form theſe buds or hybernacule in the open air during the courſe of our ſummer, will not ſuffer from the ſeverity of our winter. In the hot-houſe many plants complete their ſhoots that would not probably do ſo in the open air, the heat not being ſufficient to cauſe them to grow with the vigour neceſſary for their completion before winter. Nevertheleſs many of theſe, if not all, might be brought, by enuring them to the open air, to bear our climate. The Camelia Japonica Thea viridis and Calycanthus præcox, which were formerly kept in the hot-houſe, then in the green-houſe, are now ſufficiently naturalized to grow in the open air, and are as little injured with the cold of our winters as either the common or Portugal laurels.

Some exceptions to this obſervation ſeem to preſent themſelves. The Robinia Pſeudo Acacia (two-thorned Acacia) does not form external hybernacule, nor complete its ſhoots, yet grows well in our climate; it however, when the froſt comes on early, loſes a great part of its ſummer ſhoots. Several ſpecies of the Ciſtus, that ceaſe growing on the approach of winter, but form no hybernacule, live through our mild winters, but ſuffer greatly

in ſevere froſts. And others, as the Lauruſtinus (Viburnium tinus) continue to ſhoot and flower, unleſs the froſt is ſevere throughout the winter, ſuſtained by their vitality, or that principle whoſe exiſtence preſerves plants unhurt by cold before flowering, but which ceaſes to exiſt when the parts of fructification have performed their office *.

The Lauruſtinus is one of thoſe plants that were introduced to Ireland before green-houſes were known, conſequently planted in the open ground, and experience ſhews that it is ſeldom hurt by froſt. By it we find that ſome plants, which to appearance are not fitted for our climate, do yet outlive our winters; and that, without a knowledge of their native ſtations, we may ſometimes ſuppoſe plants to be tender which are really hardy: thus the Lauruſtinus is unhurt by froſt in Ireland until the cold exceeds that of its own climate. The Buddlea globoſa and Fuſchia coccinea are other inſtances of plants, that without a knowledge of their native climate, Chili, we would not ſuppoſe capable of being naturalized to ours. Yet is the Buddlea ſeldom injured by our cold, and the Fuſchia, although killed to the ground by the winter's cold, ſends forth abundance of ſhoots which attain the height of three feet in ſummer, and are decorated with its elegant flowers, which are larger and much more brilliant than ever they are when confined in a houſe.

And

* See Smith's Tracts, page 177, and Philoſophical Tranſactions for 1788.

And there is little doubt but many plants of Chili, and even thoſe which grow within the tropics, when found near the elevation of perpetual froſt, would bear the cold of Spitzbergen; for on the tops of mountains are found the plants of the plains of more northern latitudes. Thus is the Salix herbacea of Lapland and Spitzbergen found on the tops of Mourne mountains at about the elevation of 2,500 feet. On the Serra of Madeira, latitude 32°, 38′, and elevated 5,162 feet, is found the Erica Arborea, of the neighbourhood of Genoa, latitude 44°, 25′*, Therefore as the temperature which prevails at the elevation of 5,162 feet, in latitude 32°, is found nearly to correſpond with that of 51° north: the Erica Arborea, which grows at that elevation in latitude 32°, will find a climate ſuited to its nature in latitude 51° †. But as the before mentioned plants have a conſiderable range of latitude, it may be cultivated farther north when the ſoil and ſituation are favourable. At James Holmes's, Eſq. on the eaſtern ſhore of Carrickfergus bay, four miles north of Belfaſt, there is a plant in the greateſt vigour at the preſent time (July 1799) which has now ſtood uninjured three as ſevere winters as Ireland ever experienced, viz. 1794, 5, 1797, 8, and 1798, 9.

The

* See Sketch of a Tour on the Continent, by J. E. Smith, M.D. F.R.S, &c. page 200, Vol. I.

† On dividing 15,577, the height of perpetual froſt at the equator, by the difference of the temperature above and below, it is found that every 299 feet of elevation

The ſituation is however favourable, being againſt a weſtern wall facing the ſea, and well ſheltered by diſtant trees from ſtrong winds. And in the neighbourhood of the ſea I have little doubt but it would grow ſtill farther north.

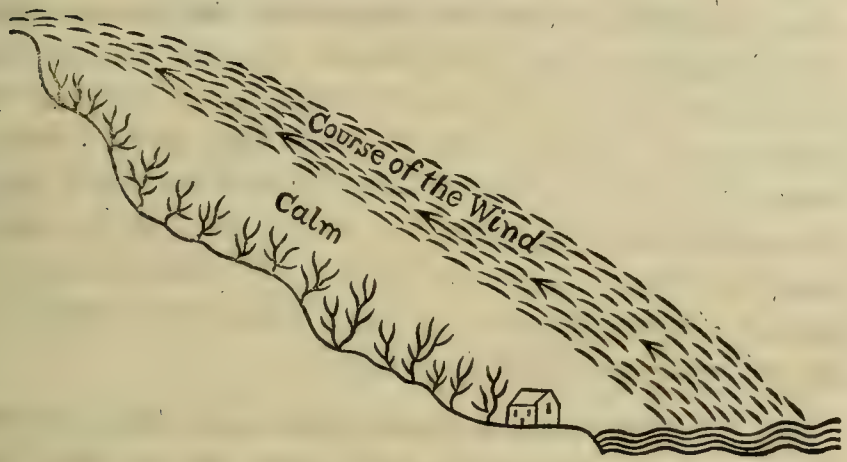
The ſea air has generally been reckoned a powerful obſtacle to having plantations on its ſhores. But many obſervations have convinced me that it is the wind alone which prevents the growth of trees on the ſhores of the ſea. And that on a large plain, where the winds are unimpeded in their courſe, the ſame difficulty of raiſing plantations as on the margin of the ocean will be experienced.

In Forſter's Account of Cook's ſecond Voyage, it is mentioned, that the trees on New Zealand were growing ſo cloſe to the edge of the water, that the ſhip's maſts were entangled among their branches; and in particular ſituations the ſame proximity of trees to the ſea might be obſerved in various latitudes. At Fairhead, the moſt northerly extremity of Ireland, and expoſed to the fury of the Northern Ocean, the Sorbus aucuparia (Mountain Aſh), Betula alba (Birch), Quercus Robur (Oak), with other indigenous trees, grow luxuriantly within 15 or 20 yards

vation leſſen heat 1°, and on dividing 5,162 feet by 299, we have 17°, which ſubtracted from 69 mean annual temperature of latitude 32°, give 52, for the Serra of Madeira, correſponding with the latitude 51° (1).

‡ See Kirwan's Table of mean annual temperatures.

yards of high-water-mark. The reaſon of this appears to be that they grow upon the lower part of very high land, which cauſes an eddy to be formed about them when the wind blows from the ſea; and by the ſame high land they are protected from the ſouth and ſouthweſt winds. The ſituation might be repreſented thus:



On the top of the rocks the wind rages with the greateſt fury, even the graſs ſeeming blighted, whereas below the rocks every plant appears in a thriving ſtate, and ſome houſes ſituated on the lower part never have their thatched roofs diſturbed by the ſtorms. In every other part along the coaſt where land is of the

the ſame form it is covered with thriving wood, but where the land is nearly level for a length of way inland no wood appears, and every hedge is ſeen never to riſe higher than the top of the bank which protects it from the wind. Therefore in order to plant near the ſea on a low ſhore, it is neceſſary to commence the plantations a conſiderable way inland, and to allow the young trees to have others ſeveral feet taller than themſelves behind them: theſe will have the ſame effect as high land, for by means of the oppoſition offered by innumerable ſtems and branches the force of the wind will be greatly leſſened; as we may find by ſtanding on the windward ſide of a thick wood during a ſtorm, where, if the trees are lofty, the wind is much leſs violent than on an open plain. In water the effect of this kind of oppoſition is viſible, for if into the bed of a ſwift ſtream we drive a number of ſtakes, the water, although it continues to flow, yet has its velocity diminiſhed conſiderably.

Our firſt plantations in an expoſed place ought always to be of ſuch trees as are natives of mountains, for theſe are fitted by nature to bear the rude blaſts of winter, and by the ſtiffneſs of their leaves, or flexibility of their footſtalks, to remain uninjured by a ſummer ſtorm. Of the firſt, we have the various race of pines; of the laſt, the Birch, the Aſpen and the Mountain Aſh *.

Thus

* Among the rocks of Agnew's Hill in the County of Antrim, I found the Populus tremula (Aſpen Tree) growing luxuriantly on the eaſtern face, at about the

Thus by a careful inſpection of the operations of nature, is the hand of man enabled to collect the productions of diſtant countries around his home, cover the arid heath with waving green, and make the lonely wilderneſs aſſume a pleaſing gloom.

the elevation of 1,450 feet. And on the top of Slemiſh, the Sorbus aucuparia. (Mountain Aſh) expoſed to every ſtorm at the elevation of 1,398 feet.

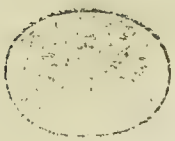


Fig. 1.

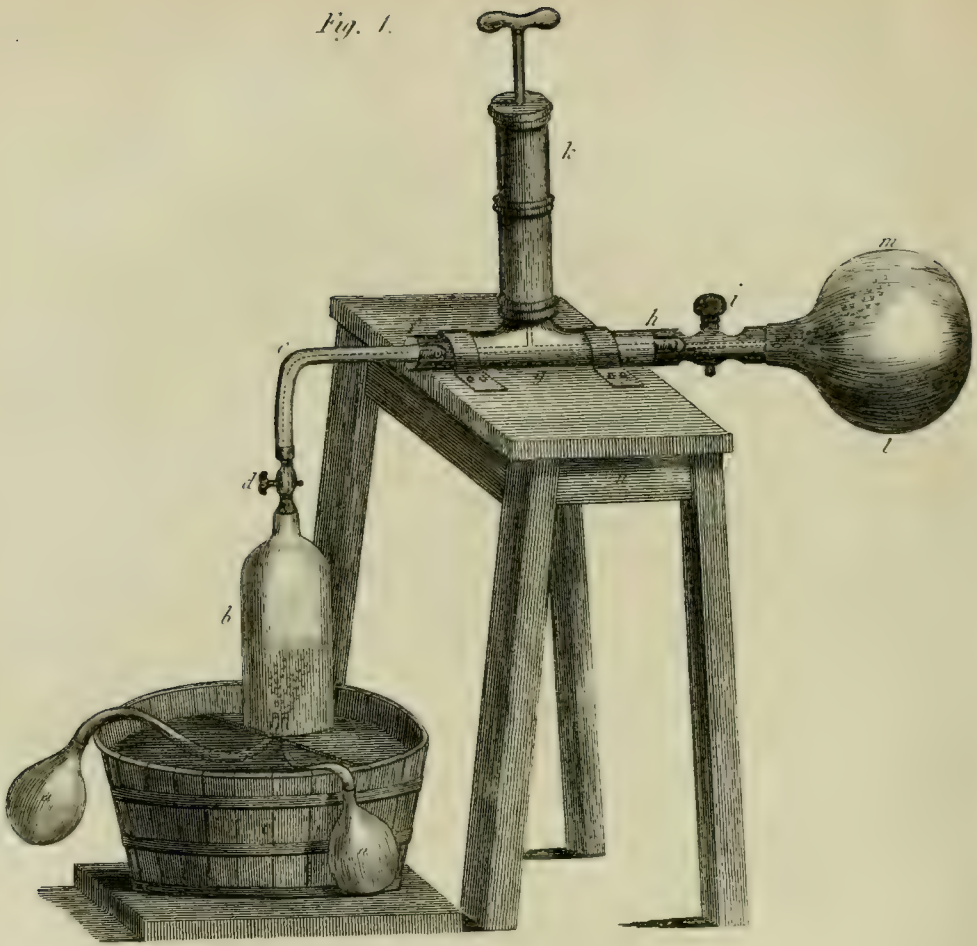
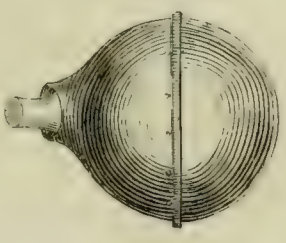


Fig. 2.



DESCRIPTION *of an* APPARATUS *for* IMPREGNATING WATER *and other* SUBSTANCES *ſtrongly with* CARBONIC ACID GAS. *By the Rev.* GILBERT AUSTIN, *M. R. I. A.*

Read Nov. 16th, 1799.

THE carbonic acid gas, or other gas, is generated in phials *(a, a)* with bent tubes in the uſual manner, and received in the jar *(b)* placed on the ſhelf of the pneumatic tub. When the jar *(b)* is filled, the phials are applied to others, which are filled alſo, and reſerved to repleniſh the jar *(b)*. To this jar it may be convenient to fit the ſtop cock *(d)* communicating with the bent tube *(e)*, or the bent tube *(e)* may be uſed without the ſtop cock, and be fitted immediately into the neck of the jar. At the end *(f)* of the bent tube is a valve opening into the tube *(g)*, and at the oppoſite end of the tube *(g)* is another valve *(h)* opening into the ſtop cock *(i)*, which communicates with the ſtrong glaſs veſſel *(l, m)*. To the tube *(g)* is adapted the condenſing ſyringe *(k)*, which is placed between the valves *(f* and *h)*. The tube *(g)* is firmly fixed to the ſtand *(n)*, ſo that the condenſing ſyringe may be worked without diſturbing the apparatus: all the joints muſt be well fitted and perfectly air-tight.

The uſe of this apparatus muſt be obvious. The parts are all to be ſcrewed together as in the figure except the ſtrong glaſs veſſel and its ſtop cock; the phials are alſo to be removed. The jar *(b)* being placed on the ſhelf of the pneumatic tub will, in theſe circumſtances, contain a portion of the common air of the apartment. In order to remove this, the piſton of the condenſer is to be raiſed; upon which the valve at *(b)* will cloſe, and that at *(f)* will open and admit into the vacuum formed in the barrel a portion of the common air contained under the jar, and the water will riſe in the jar in proportion to the capacity of the barrel of the condenſer. On thruſting down the piſton the valve *(f)* cloſes, and the air is expelled through the valve *(b)*, This operation is repeated till all the common air is removed from the jar *(b)*, and the water completely fills it. The ſtrong glaſs veſſel *(l, m)* with its ſtop cock is now ſcrewed on. It is nearly filled with the water to be impregnated, but not quite, a ſmall portion *(m)* of air being left in order to allow room for the agitation of the water. The jar *(b)* is now to be filled with carbonic acid gas, which is pumped up and forced into the glaſs veſſel in as large a quantity as it will admit without danger of burſting, which in my veſſel containing about a pint meaſure is about an equal bulk of gas The glaſs veſſel is then to be taken off (its ſtop cock being previouſly cloſed) and agitated briſkly; and as the water abſorbs the gas more readily when many of its particles are thus forced into contact

contact with it, the condenſation may be repeated ſeveral times. It appears uſeful in order to expel the bubble of common air left in the veſſel for the convenience of agitation, after two or three condenſations to open the ſtop cock and ſuffer it to be expelled, and then to renew the charge of the carbonic acid gas. In a very few minutes water may thus be acidulated to any degree (provided the apparatus is ſufficiently ſtrong and cloſe) ſo as even to foam out of the glaſs veſſel like liquors highly in bottle as ſoon as the ſtop cock is opened.

As the carbonic acid gas may be quickly generated or preſerved for any time in bottles cloſely ſtopped, artificial mineral waters in great perfection may by this apparatus be prepared as ſoon as any other medical preſcription.

The apparatus I have conſtructed is of braſs, and however carefully cleaned perceivably imparts a taſte of the metal to the water. It is adviſeable therefore to make it of ſome metal which either may not poſſibly impart any ſenſible taſte to the water, or which may not be conſidered injurious even if diſſolved in it in very ſmall portions; ſilver or tin appear beſt adapted for this purpoſe, but as the former may be too expenſive for general uſe, and as the latter is too ſoft to bear well the frequent ſcrewing and unſcrewing neceſſary in the uſe of the apparatus, the channels through which the gas paſſes may be well coated with ſilver or tin, which may anſwer the purpoſe; the pipes, and cocks may have a thick ſilver wire ſoldered in the centre which

may

may be perforated, and the condenſer may be well plated or tinned on the inſide.

But as glaſs appears to be the moſt unexceptionable material, I have ordered an apparatus to be conſtructed entirely of glaſs, and have hopes that I ſhall have it ſo executed as not to admit any thing elſe to come in contact with the gas. As the difficulty of execution may however cauſe a variation in my plan, I ſhall defer the laying it before the Academy till I get it accompliſhed to my wiſh. Ingenious workmen may alſo be induced to contrive and execute ſome apparatus which may anſwer the purpoſe, ſhould my ideas ſeem practicable to them or uſeful for philoſophic experiments.

As diſagreeable accidents might ariſe from the burſting of the glaſs veſſel, an ingenious mechanical friend has ſuggeſted to me the propriety of encloſing it in a ſtrong copper caſe. It may conſiſt of two hemiſpheres with a broad rim ſcrewed together as in figure 2.

ANALYSIS *of* TURF ASHES. *By* LORD TULLAMORE, *M. R. I. A. Communicated in a Letter to the Rev. Doctor* ELRINGTON.

My Dear Sir,

GIVE me leave to ſubmit to you ſome experiments I happened to make in the courſe of laſt ſummer, during my reſidence at Charleville Foreſt.

Read, Nov. 23d, 1799.

I am tempted to communicate them, becauſe their reſult to me appears ſomewhat curious, though very different from what I wiſhed or expected; and I leave it to your better judgment to decide whether ſuch puny efforts are worthy even for a moment, the conſideration of the Royal Iriſh Academy.

While Bogs predominate ſo generally though this iſland, they muſt attract univerſal attention, at leaſt in an agricultural point of view; and every ſucceſsful effort towards reſtoring them to a ſtate of fertility is of great national importance.

My

My little experience inclines me to think, that one of the moſt advantageous, if not indiſpenſable preliminary ſtep in reclaiming bog, is ſubmitting it to the proceſs of burning, in a manner not very different from the common practice; by which, if I am not miſtaken, a greater quantity of aſhes may in moſt caſes be with eaſe procured from a given ſpace, than is eſſentially neceſſary to its future fertilization.

It were a very deſirable point gained, could we appropriate their ſuperfluity to any uſeful purpoſe, conſidering the inexhauſtible ſource it would afford, and the double advantage of its operation; and I might have eſteemed myſelf fortunate, had I been able to obtain from it, an article we have hitherto ſought from abroad although of prime neceſſity to our moſt flouriſhing manufactures.

We are taught by the obſervations of moſt experimental chemiſts, that aſhes of all vegetables afford more or leſs Potash: and conſidering bog or peat as now univerſally allowed of vegetable origin, I was led by analogy to ſuppoſe, that, after it had undergone a ſimilar proceſs of incineration, a ſimilarity of product, though proportionally ſmall, might be the reſult.

With a view ſolely to prove how far ſuch reaſoning was founded, I had a quantity of turf, free from wood or any other combuſtible materials whatſoever, burned under my immediate inſpection. When I had procured ſufficient aſhes, and ſifted them

them, I poured on boiling water, thus by lixiviation to extract any ſaline matter from the earthy or leſs ſoluble parts. I ſuffered this mixture, repeatedly ſtirred, to ſubſide for about twelve hours and the clear liquor decanted off, which had already a brackiſh taſte, was ſoon evaporated down to a ſtrong ſaline brine.

In order to ſeparate the cryſtallizable ſalts from the *potaſh*, which under ſuch circumſtances would not cryſtallize but remain in ſolution in the mother liquor, I ſet this brine to cool; and, on examining it the following day, found an abundant crop of beautiful cryſtals, ſhot out to a conſiderable ſize, a few of which I have preſerved for your inſpection.

The remaining liquor when evaporated to dryneſs afforded a ſaline maſs in conſiderable quantity, which, from its ſtrong effect on vegetable blues, I ſtill hoped might contain ſome of the deſired *alkali*; but on ſubmitting it to the examination of my ingenious and learned friend Mr. Higgins, he could not detect a particle of any kind of diſengaged *alkali*, though he ſought it by ſome of the niceſt teſts of modern chemiſtry. I therefore think we are founded in concluding, that nearly the whole maſs of ſalts thus procured muſt conſiſt of *ſulphat* of *ſoda* with little or no intermixture.

I confess, my dear Sir, that regarding this reſult in a philoſophical, but particularly in a geological point of view, it appears to me not a little ſingular, that *marine alkali* combined with

ſulphuric acid ſhould be found in ſuch abundance in turf aſhes procured at a great diſtance from the ſea; but my admiration encreaſes when I alſo take into conſideration the very great ſolubility of this compound; and (if we allow the exiſtence of ſalts in vegetable matter previous to combuſtion) the length of time it muſt have remained without being operated on, in a ſubſtance eternally pervaded by water.

THAT ſuch a ſalt may be thus procured has not, to my very limited knowledge, hitherto been noticed by any writer. Doctor Darwin mentions the aſhes of peat, and denies indeed their containing alkali; and Mr. Jameſon, who gives an analyſis of *unburned* peat, did not produce *ſulphat* of *ſoda*, though he obtained *ſulphat* of *magneſia*, of which ſome ſuppoſe the former a modification.

I DID intend to have purſued the inveſtigation of this ſubject at future leiſure, but am very happy to find it is already in much abler hands; I have therefore only to entreat your indulgence for treſpaſſing thus far on your time, and to aſſure you that I am,

My dear Sir,

Your faithful humble ſervant,

TULLAMORE.

Blackrock, October 16th, 1799.

P. S. I was induced, ſince I wrote the above, to try the aſhes of a different though neigbouring bog which were of a deep

red

red colour, whereas thoſe I had at firſt uſed were *white*. The reſult varied; as the red aſhes produced the *muriate* of ſoda only, but in much ſmaller quantity than I had before procured of *ſulphat* of ſoda from the *white* aſhes, which was about one ſixty-fourth of the original weight. This appeared to me rather intereſting, as it may in ſome degree account for the higher efficacy of *red* aſhes as a *manure*. I alſo find that *ſulphat* of ſoda has been already detected in turf aſhes. Vide Mem. Bruſſels. Vol. III.

A MEMOIR *of the* MINES *of* GLAN, *the* ROYALTY *of* RICHARD MARTIN, *Eſq. By Monſieur* SUBRINE, *Engineer to the King of France.*

Read, June 1800.

THE territory of Oughterard is covered to the eaſt by a calcareous ſtone of a blueiſh colour, ſhelly and fœtid, diſpoſed in layers parallel to the ſurface, and ſuſceptible of a very beautiful poliſh. To the weſt it is covered by a roſe-coloured granite, which however varies ſometimes to a green, the layers of which are moſtly perpendicular to the horizon, and are eaſily decompoſed by the air. To the ſouth it is bounded by the mountains of Mahiramore, the ſummit of which is compoſed of many conſiderable layers of quartz of a very white colour: but the baſe of the mountains is formed of alternate layers of very hard ſlate, and is a ſort of quartz more or leſs denſe. To the north it is bounded by the mountains of Glan, which are entirely compoſed of ſlate, and in which the mines, that make the principal object of theſe obſervations, are found.

THE

The reſpective ſituations of theſe different ſubſtances are ſingularly perſpicuous at the bridge of Oughterard. The caſcade which the river forms in that place, ſeparates the granite from the calcareous country. The precipitate motion of the torrent has ſtripped and dug up the ſoil to ſuch a degree, as to expoſe each layer or ſtratum of the rock, and ſhew them arranged in a ſymmetrical order, ſeldom to be met with in the irregularities of nature. The grotto which has been conſtructed near that caſcade, is cut into the very ſpace that divides theſe two ſyſtems. Here one may ſee three or four beds of ſlate, which are laid immediately over the granite, and which are covered over with calcareous layers, perfectly parallel to them. Thus we learn the order in which the operations of nature were performed. By this ſort of obſervation we are taught to know that the granite was the ground-work of the globe; that the ſlate was afterwards formed and laid over it; and that finally the calcareous ſtone, the moſt modern of all, was laid over the latter. We muſt here remark, that the calcareous ſtone we are now talking of, is that wherein one meets an infinity of ſhells. As to that ſpecies of it which is abſolutely pure, without the apparent remains of ſea-animals, ſuch as are to be ſeen in the mountains of Glan, and which are alſo to be ſeen in the very high mountains of the Alps, it is not as yet decided whether we are to conſider thoſe as contemporary to the granite or to the ſlate, or whether their origin be not of the ſame date with the preceding claſs. In order to diſtinguiſh between

between thoſe two ſpecies of calcareous ſtone, it will be ſufficient to call the firſt the ſhelly calcareous, and the ſecond the pure calcareous. The ſhelly calcareous ſtone, though alway laid on the ſlate, is not the only ſtone which occupies that place; for we ſee that the lower part of the mountains, to the ſouth of Oughterard, exhibits alternate layers of quartz and ſlate, and that their ſummits conſiſt entirely of quartz. The ſame obſervation is to be made on St. Etienne en forez in France. On the ſide of Mount Bilat, a heap of ſlate is to be ſeen, called Roche Coupée, which riſes to the elevation of from 60 to 80 feet on the ſlaty top of the mountain, and exhibits very diſtinct beds like thoſe of Mahiramore. The black mountains in lower Brittany contain ſuch beds of quartz as form in themſelves mountains over the ſlate. Mr. Patrine, a french naturaliſt, who went to ſee the mines of Siberia, aſſures us that he has ſeen ſome exactly like them in the north.

The mountains which ſurround the bay of Dublin on the north are very intereſting; for upon examination they are found to conſiſt of ſlate, in which conſiderable beds of quartz predominate, at one time very pure, and at other times more or leſs argillaceous, but generally like what the French mineralogiſts call petroſilex. The mountains bordering the ſea on the coaſt of Holyhead, facing Dublin, are abſolutely of the ſame nature with thoſe laſt mentioned; but as it is not our object to compoſe a theory

theory on this ſubject, for which work indeed it does not appear that any naturaliſt has hitherto collected ſufficient materials, and as we would conſider it raſhneſs in us to attempt ſuch an undertaking, conſidering the ſhort ſpace of time we had to inveſtigate matters, we will confine ourſelves to the following obſervations:

1ſt. The granite that forms the ſouth ſide of the mountains of Glan, and extends acroſs the river of Oughterard as far as the foot of Mahiramore, does not ſeem to be covered or interrupted any where with any other ſubſtance.

2d. It is not interſected by any perceptible vein; but one may remark, about two hundred paces above the bridge of Oughterard, ſome veins or threads of ſpar, ſo minute as to be almoſt imperceptible, through which ſome cryſtals are interſperſed, whoſe greateſt thickneſs does not exceed half an inch.

3d. Thirty feet above the river, on the right ſide, there are ſeveral beds of granite, which ſeem to have been fractured by ſome violent force. The ſpace between their numerous ruptures is filled with ſpar, which holds a great quantity of lead.

4th. All the country in the neighbourhood of Oughterard abounds with ſcattered lumps of rolled granite, ſome of which are

are more than ſix feet in diameter. On the top of the mountain there is one that is monſtrous. It is placed on the very ſummit; and it is ſo placed, that though it is nine or ten feet in length, it has not four in the baſe: all thoſe ſtones are of the ſame nature with the fundamental granite of the country.

5th. Immediately above the granite towards the grotto of Calypſo three or four beds of ſlate are to be ſeen, the uppermoſt of which is ſo chequered with quartz that it may be eaſily miſtaken for argillaceous clay.

6th. Above theſe beds of ſlate are to be ſeen the calcareous ſhelly beds which are extended down the river towards Loughcorreb, and continue ſome miles on the Galway road.

7th. Near Portcarren you can diſcover a very thick vein of calcareous ſpar, in which you will find ſome lead mixed with blackjack. It is impoſſible to bring up any level to it, as it is as low as the lake.

8th. Above the granite, on the ſouth ſide, begins the ſlate mixed with quartz, which forms the foot of the mountain of Mahiramore.

9th. About half way up the mountain the ſlate is very argillaceous. On the ſouth ſide of Oughterard it contains ſome hardly

ceptible veins of lead, and on the oppoſite ſide it appears there is a lead mine, which was once worked, but is now abandoned.

10th. The entire ſummit of the mountain, for a length of more than a thouſand, and a breadth of about an hundred feet, is of a very pure quartz, very white, and ſometimes of a green yellow, diſpoſed in beds parallel to the ſouth top of the mountain.

11th. At the foot of the mountains that are to the eaſt of Mahiramore, by the ſide of the Galway road, at the ſpot called Bunnagippane, there are beds of pure calcareous ſtone perpendicular to the horizon, and of a colour almoſt as white as alabaſter. They are penetrated in different directions by veins of calcareous ſpar, the cryſtals of which, having reſiſted the diſſolving of air and water, remain jutting out of the ſurface of thoſe beds, ſo that one may be tempted to endeavour to diſengage them with the hand; but they ſtick ſtrongly and deeply. Theſe veins of calcareous ſpar are throughout very pyritous, and the body of the rocks conceals a good deal of pyritous ſubſtances, cryſtallized into very perceptible cubes.

12th. In that pure calcareous ſtone of Bunnagippane a load of lead was diſcovered, but without the leaſt appearance of a vein.

13th.

13th. It would be neceſſary to examine whether that pure calcareous ſtone be fixed on the ſlate, as the calcareous ſhelly ſtone is; or whether it be interpoſed in beds, or in veins among the ſlate, as it ſeems to be at Glan; or whether it forms a particular ſyſtem like the granite.

14th. Above the granite to the weſt, at about two miles diſtance from Oughterard, at a place called Lettercraft, there is a ſlate very like that at Glan, in which it was pretended a vein of lead was diſcovered. We have in truth obſerved ſome quartz and argillaceous veins expoſed by the current of the river; but it is impoſſible to ſay any thing more of it at preſent. The pieces of lead which we carried from that ſpot, came from the trials that have formerly been made there.

15th. On the banks of the river of Oughterard, both in the granite and calcareous parts, one may obſerve an ochreous ſediment or depoſite, often very hard and thick; which made people imagine that it was contiguous to ſome iron mine: the ferruginous taſte of the little fountain which runs above the grotto of Calypſo ſtrengthens this opinion; but as the entire valley traverſed by the river of Oughterard is covered with turf to a very great depth, and as this bog or turf, being decompoſed in the boſom of the marſhes, muſt communicate to the waters which traverſe it per-

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petually the iron with which the vegetables are more or leſs impregnated, it will not appear ſurprizing that the river of Oughterard, as well as the neighbouring fountains, ſhould be ferruginous. Theſe ochreous ſediments are therefore only the dregs formerly abandoned by the river when it was of a greater extent on the banks, or the immediate products of the decompoſition of the turf, of which the place is now ſtripped.

16th. We have ſaid in ſection ſecond, that the ſouthern ſides of the mountains of Glan were granite. The northern ſides are entirely ſlate. It is in the ſlates that the different veins, commonly known by the name of the mines of Glan, are wrought, or rather undertaken, which we are about to deſcribe in regular ſucceſſion. Firſt, however, allow us to ſay a few words of the country in which they are found.

17th. The mountains of Glan form a chain round Loughcorreb, and dip into it under an angle of about forty degrees. Their ſummit viewed from the lake may be near four hundred feet of perpendicular height. It is in the middle of this height, and in the lower parts of it, that all the mines have been diſcovered.

18th. The ſlate of which they are compoſed is diſpoſed into very regular beds, inclining forty-five degrees at one time in the direction

direction of the mountain; at another time in a quite contrary direction; and as the mountain has different vallies, as you turn from one to another you diſcover the ſlate in all poſſible directions.

19th. Those ſlates are at one time tender and leafy—at another time hard and very compact—they are intermixed with beds of quartz, the grain of which reſembles the petroſilex greatly. The hard ſlates have a reſemblance to what the Germans call *hornblende*, and what the French call *ſhorl en roche*, and to what they both call *gwiſs*.

20th. All the loads are traverſed in certain places by beds of pure calcareous ſtone, pretty white, which is one time parallel to the horizon, as at the top of the hill, which is between the third and fourth vein; at another time inclined from forty-five degrees, as in the ſhaft put down on the fourth vein.

21ſt. One obſerves in thoſe calcareous beds the ſame pyritous and ſparry veins that we have deſcribed in ſection the ſecond.

22d. One ſhould determine whether calcareous ſtrata are interpoſed in a parallel manner in the ſlates, or if they cut them like veins, or if they form a particular ſyſtem on the ſides of the ſlates.

23d.

23d. At the place you quit the road of Glan to enter into the mountain, and near the lake, there are traces of different trials, but all now abandoned. Some are on veins of lead, others on pyrites, their ſituation being altogether in the plain. The impoſſibility of bringing up levels to them, has, no doubt, occaſioned them to be abandoned. We mention them here only to ſhew how abundant in mines this territory is.

24th. The firſt trial we propoſe to examine is that called Ambroſe's Shaft. It is ſituate about two hundred feet above the lake in a very ſteep place. The vein at this work is almoſt perpendicular to the horizon. The direction is oblique to the mountain, ſo that ſuppoſing it regular and continued, it would extend in a diagonal line from the top of the mountain to the lake. It is about two inches thick. Near Ambroſe's Shaft it forms a ſolid bed of quartz, ſo detached from the mountain as to make one preſume it is ſo throughout the entire of its courſe. The copper ore is found to be contained in the quartz, and mixed with a great quantity of ſulphur. The trial in that place has been abandoned on account of the ſcarcity of the ore, and the hardneſs of the vein.

25th. We conſider that the only way to work to advantage on this vein would be to open a gallery at the lowermoſt part of the mountain in the very vein itſelf, which according to its apparent

apparent direction ought neceſſarily to tend thither; and in caſe it could not be diſcovered to open one in ſome degree oblique to the known, and already diſcovered courſe of the vein. Since the compilation of this article we viewed the mountains more particularly, and have found the vein, in coming down in an oblique line as far as the lake traverſes above Ambroſe's Shaft a little valley, remounts to the oppoſite hill, and is finally loſt on the other ſide in the mountain which immediately borders on the lake. The rivulet that flows through the valley has opened the ground ſufficiently to let one ſee the direction of the vein clearly. If a gallery was opened in that place, and proſecuted in the very vein to the right and to the left of the rivulet, one would probably diſcover the vein; but as the rivulet runs down the hill obliquely, and as the courſe of the vein inclines a little in the ſame direction, we believe it would be better to broach the gallery at the very bottom of the valley, taking care however to proſecute it in a direction as much at right angles as poſſible to the courſe of the vein; and when this gallery ſhall reach the courſe another gallery ought then to be driven in the vein itſelf to the right and to the left, and as a greater depth will be reached in proportion to the extent in which the work is proſecuted in this way. By theſe means, we are of opinion, that an effectual trial may be made of this load.

26th.

26th. The ſecond trial called the New Vein, which we will call the Cottage Vein, offers more facility than the former, but in return the ore found here is not ſo rich. In order to conceive the matter diſtinctly, one muſt obſerve, that the beds or layers of the mountains are inclined in this place about forty-five degrees, and a vein of quartz, almoſt perpendicular to the horizon, cuts them very regularly between the vein of quartz and the layers of the mountain. On the ſide hanging of theſe a light layer of argillaceous or potter's clay covers the vein and travels with it in its deſcent. This vein has not been broached; but among the beds of ſlate which deſcend at its right, there are two totally pyritous in which a gallery was commenced. Three layers of pyrites are here worked out as copper ore, and really, though they are very ſulphureous, they contain, notwithſtanding, metal ſufficient to ſupport and cheriſh the hopes of a miner.

27th. At an hundred feet perpendicular height above this trial, near an half-finiſhed pavilion called the Cottage, the ſame pyrites are found again in beds of ſlate like thoſe we have juſt ſpoken of. Every motive induces us to believe that they are a continuation of the ſame vein, and we are of opinion that it would be expedient, for the purpoſe of aſcertaining this point, to lay open that ſpot completely, and examine whether or not the vein of quartz, of which we have already ſpoken, is to be found there.

28th.

28th. If the vein of quartz is to be found towards the pavilion, then we may be ſure of the regularity and fertility of the load; for the pyrites appearing in abundance at both extremities of the vein, the heart of the mountain, in which it is evident the vein is fed, ought neceſſarily to be very rich.

29th. At all events, the beſt trial that can be made on that place, will be to make an horizontal drift into the mountain, and following the vein of quartz; and in the courſe of this operation, the bed of pyritous ſlate will be cut through. If the vein of quartz ſhall be found to be metallic, this work will turn out advantageouſly; but if it ſhould not be found ſo, one ought then to examine whether the layers of ſlate, which lean againſt it oppoſite to the work, contain any ore.

30th. The trial called the open caſt does not appear to be a regular vein; it is a confuſed heap of blueiſh ſlate of quartz, and a kind of petroſilex, through which pyrites and yellow copper ore are ſpread with great irregularity. That has however, towards its top, an appearance of inclining in the direction of the mountain, that is an angle of about forty-five degrees; and it is covered with a layer of quartz, which at firſt looks like a vein, but which, when accurately examined, is found to be nothing more than a heap joined to the body of the load, and inclined like it in a plane parallel to the ſurface.

31ſt. A SHAFT, called painters ſhaft, was ſunk on this load, about four or five fathoms in depth, which produced ſome ore; but as the ore was miſſed, it was thought the vein was loſt, and in hopes to find it again, they went off on the ſlide bottom of the ſhaft in the direction of the mountain. This project, after it was proſecuted for a fathom or two, was abandoned, for the rock they met was found abſolutely of the ſame nature with the rock at the ſurface of the mountain in the broil of the load.

32d. AFTER this fooliſh trial, they again attacked the ſurface of the mountain, and at the diſtance of twelve ſteps from this ſhaft they opened another ſhaft on the broil of the load, which was dropped, after proſecuting it four and a half fathoms in depth, on account of the water which flowed in abundantly; and to eaſe this water, a little draining level was opened in the ſide of the mountain, which joined the ſhaft at the bottom, where they both reached the quartz. It was intended to proſecute this ſhaft to a greater depth, and it was hoped the water would no longer obſtruct the work, for it was imagined to have been all top water.

33d. IT is eaſy to ſee the error fallen into in the conduct of painter's ſhaft. To find any ore, the miners concerned in this work might well have judged that they had loſt the load, and then

then ought, inſtead of beginning a ſecond ſhaft and bringing up a level to it, to have made drifts at the bottom of the ſhaft to the north and to the ſouth: to the north until they came out to day, which drift would have ſerved to drain off the water; and to the ſouth until they had penetrated into the mountain far enough to recover the load again.

34th. And this is ſtill what in our opinion ought to be done; for the ſecond ſhaft and its level, being only in the brol of the load, are perfectly uſeleſs; and the firſt ſhaft which is entirely in the body of the ore, and much deeper than the other, is well circumſtanced for exploring the richneſs of this load; in conſequence of which we think it will be diſcovered.

35th. If upon this trial it ſhould be found that the ore is ſcarce or poor, it will then be right to continue the ſhaft and make a trial of the load at a greater depth: and for this work it will be neceſſary to provide pumps, called ſupplementary or eſtabliſhed buckets, of a ſufficient ſize to keep the water free.

36th. This may not be an improper place to remark, that the probe is an inſtrument often fallacious in the riches of mines, for metallic beds obſerve no regularity, and their gage never being homogeneous, the probe, which reaches only one ſpot,

 may

may very well reach the ore, and yet carry back only a very uncertain intelligence. The ſterile layers of the mountain contain ſcattered ſubſtances exactly like thoſe that in the veins may be taken as a gage to the ore, and in the moſt metallic veins one meets with ſome ſtrings of ſlate, or of other rocks, abſolutely of the ſame nature with the mountain: it is evident that the probe, in carrying back with it both the one and the other of thoſe matters, cannot give compleat ſatisfaction.

REMARKS *on ſome* SCEPTICAL POSITIONS *in* MR. HUME's ENQUIRY *concerning the* HUMAN UNDERSTANDING *and his* TREATISE *of* HUMAN NATURE. *By* RICHARD KIRWAN, *Eſq; L. L. D. F. R. S. and P. R. I. A.*

Read, Oct. 20th 1800.

THOUGH numberleſs treatiſes of the ever jarring ſects of ſcholaſtics had in all countries and for ſeveral ages obſcured and diſgraced the ſublimer regions of ſpeculative philoſophy, and have been the fruitful parents of many abſurdities in the ſciences connected with it, yet, to the honour of the philoſophers of that denomination, it muſt be owned, they confined their reaſoning rage within the limits of pure ſpeculation, and refrained from meddling with thoſe principles that have an immediate influence on the conduct of human life. We have lived however to ſee an attempt made in our own days by a modern philoſophical ſect to ſubvert theſe alſo, or at leaſt to involve them in all the darkneſs of the moſt dreary ſcepticiſm. For after the miſts that overſpread the regions of mere ſpeculation had

had been in great meaſure diſſipated by the luminous reſearches of Mr. Locke towards the end of the laſt, and of Dr. Berkeley towards the commencement of the preſent century, Mr. Hume about the middle of it advanced and diffuſed, with no mean degree of ſucceſs, many paradoxical opinions avowedly ſubverſive of the primary principles of human reaſon, and tending to prove that moſt of its concluſions, even thoſe of a practical nature, would, when traced to their ſource, be found deſtitute of any ſolid foundation, and in a word merely empiric. Of theſe opinions I have ſelected a few, namely:

1°. That beings of any kind may ſtart into exiſtence without the intervention of any efficient cauſe:

2°. That the connexion between phenomena and their ſuppoſed cauſes can in no caſe whatſoever be traced by reaſon, but in all caſes is inferred merely from experience:

3°. That inferences from experience are themſelves unſupported by any ſolid reaſon, for that none can be aſſigned for expecting ſimilar effects from ſimilar cauſes, but the mere cuſtom or habit of ſeeing them conjoined:

4°. That *belief* is not an act of judgment, but a particular ſpecies of ſentiment or feeling:

Lastly,

Lastly, That fallacious as all experimental reaſoning may be, yet the violations of the laws of nature (the exiſtence of which laws can be known and inferred only from experience) cannot be rendered credible in any caſe by any human teſtimony whatſoever.

On each of theſe aſſertions I mean, with the indulgence of the Academy, to make a few obſervations.

Section I.

Of the Neceſſity of Cauſes.

Mr. Hume, in the 3d ſection of the firſt book of his Treatiſe on Human Nature acknowledges, "it is a general maxim in philoſophy *that whatever begins to exiſt muſt have a cauſe of exiſtence*; this is commonly taken for granted, being ſuppoſed to be founded on intuition," but if examined, he tells us "it will be found to diſcover no mark of intuitive certainty; for this ſpecies of certainty ariſes from the diſcovery of ſuch relations as are unalterable ſo long as the ideas continue the ſame, theſe are *reſemblance*, *proportions in quantity and number*, *degrees of any quality*, and *contrariety*; none of which are implied in this propoſition, *whatever has a beginning has alſo a cauſe of exiſtence*; it is not therefore intuitively certain, at leaſt whoever aſſerts

" aſſerts it to be ſo, muſt deny theſe to be the only infallible " relations, and muſt find ſome other relation of that kind im- " plied in it*.

" We can never demonſtrate the neceſſity of a cauſe to every " new exiſtence or modification of exiſtence, without ſhewing " the impoſſibility that any thing can ever begin to exiſt with- " out a productive cauſe; now that this is utterly incapable of " a demonſtrative proof we may ſatisfy ourſelves by conſidering, " that as the ideas of cauſe and effect are evidently diſtinct, it " will be eaſy for us to conceive any object to be non-exiſtent " this moment and exiſtent the next, without conjoining to it " the diſtinct idea of a cauſe or productive principle. The ſe- " paration therefore of the idea of a cauſe, from that of a " beginning of exiſtence, is plainly poſſible for the imagination, " and conſequently the actual ſeparation of theſe objects is " ſo far poſſible that it implies no contradiction nor abſurdity, " and is therefore incapable of being refuted by any reaſoning " from mere ideas, without which it is impoſſible to demonſtrate " the neceſſity of a cauſe."

To me, however, the propoſition alluded to, namely, *that whatever begins to exiſt muſt have a cauſe of its exiſtence*, conveys intuitive

* In quoting Mr. Hume I do not always ſcrupulouſly adhere to his expreſſions; I endeavour to abridge, always retaining his ſenſe.

intuitive certainty, a certainty not grounded on the relations he mentions, but on that of cauſation implied in the very terms of which that propoſition conſiſts, for the beginning of exiſtence or the tranſition from non exiſtence to exiſtence is evidently a change from nothing to ſomething; now a change of any kind implies an action of ſome being or other, as it enters into the definition of an action, an action being ſuch a diſpoſition of a being as that a change reſults from it. But it cannot be the action of the being that begins to exiſt, as is evident: it muſt therefore be the action of ſome other being. Now a being from whoſe action the exiſtence of another being reſults, is what is denominated its *cauſe*: the relation of cauſation is therefore included in it, and inſeparable from the conception of *beginning* exiſtence.

In this argument the demonſtration reſts on the relation of cauſation neceſſarily reſulting from the conſideration of beginning exiſtence; but an argument equally forcible will be found to ariſe from the conſideration of another circumſtance contained in Mr. Hume's objection to the intuitive certainty of the propoſition in queſtion.

He tells us that, "as the ideas of cauſe and effect are evi-
"dently diſtinct, it will be eaſy for us to conceive any object
"non-exiſtent this moment, and exiſtent the next, without con-
"joining to it the diſtinct idea of a cauſe or productive prin-
"ciple. The ſeparation therefore of the idea of a cauſe from

" that of beginning exiſtence is plainly poſſible for the ima-
" gination."—Now I defy Mr. Hume or any one elſe to imagine an object as non-exiſting He may imagine one that does not exiſt, as a golden mountain and a thouſand other objects that imply no contradiction, and *judge* them not to exiſt, but he cannot *imagine* them but as exiſting; their repreſentation in the imagination is juſt the ſame whether judged to exiſt or not to exiſt. To imagine an object as *non-exiſting*, if it means any thing, ſignifies to have no imagination at all of it. This overſight is ſo much the more remarkable, as he himſelf juſtly obſerves*, that the imagination barely copies or tranſpoſes the copies of ſenſible impreſſions; now a non-exiſting object, as ſuch, can ſurely make no ſenſible impreſſion. It is however true that we can conceive an object, and *judge* it not to exiſt this moment and to exiſt in the next; but during the moment of its non-exiſtence, it muſt be ſuppoſed *poſſible*, that is, that a power exiſts ſomewhere of bringing it into exiſtence; this is implied in the very notion of poſſibility. Reference to a cauſe is therefore implied in that notion and cannot be ſeparated from it.

Mr. Hume may perhaps reply that his notion of the poſſibility of an object includes no more than the compatibility of the attributes of that object—yet ſomething more is evidently neceſſary to arrive at the ſtate of exiſtence, otherwiſe all objects whoſe

* P. 318 of the Edition in 4to.

whoſe attributes involve no contradiction, would actually exiſt, and exiſt from all eternity. What ſhould prevent them? Then we ſhould have golden mountains, flying horſes and every other chimera realized.

Section II.

Of the Source of our Knowledge of the Relation of Cauſe and Effect.

The next aſſertion of Mr. Hume which I now mean to examine, is that the connexion between cauſes and their effects can in *no caſe* be diſcovered *a priori* or by reaſoning, but is ſolely collected from experience. There is no principle on which he more firmly relies, or repeats oftener. Thus, in the 155th page of the firſt volume of his Treatiſe on Human Nature, he tells us "that the inference "we draw from cauſe to effect is not derived from a ſurvey of "theſe particular objects, and ſuch a penetration into their "eſſences as may diſcover the dependance of the one upon the "other. Such inference would amount to knowledge, and would "imply the abſolute contradiction of any thing different." And, page 299 of the 4°. edition of his Eſſays, he ſays, "I ſhall venture "to affirm as a general propoſition which *admits of no exception*, "that the knowledge of this relation (of cauſe and effect) is not in "any inſtance attained by reaſonings *a priori*, but ariſes entirely "from experience, when we find any particular objects are con-"ſtantly conjoined with each other." And, page 300, "We

" fancy that, if we were brought of a ſudden into this world, we " would at firſt have inferred that one billiard ball would commu- " nicate motion to another upon impulſe—but the mind can " never poſſibly find the effect in the ſuppoſed cauſe by the moſt " accurate ſcrutiny and examination—motion in the ſecond bil- " liard ball is quite a diſtinct event from motion in the firſt, nor " is there any thing in the one to ſuggeſt the ſmalleſt hint of the " other." And, page 301, " When I ſee a billiard ball moving in " a ſtraight line towards another, even ſuppoſe motion in the other " ſhould by accident be ſuggeſted to me, may I not conceive that " a hundred different events might as well follow from that cauſe? " May not both the balls remain at abſolute reſt? May not the " firſt ball return in a ſtraight line, or leap off from the ſecond in " any line or direction? All theſe ſuppoſitions are conſiſtent and " conceivable. Why then ſhould we give the preference to one, " which is no more conſiſtent or conceivable than the reſt? No " reaſoning *a priori* will ever be able to ſhew us any foundation " for this preference."

Now, in oppoſition to what is thus ſo confidently aſſerted, I ſay, that from a knowledge of a few of the moſt general properties of bodies, a knowledge which is implied in the ſuppoſition of an attempt to reaſon *a priori*, the fact of the communication of motion by impulſe may be inferred previous to actual experience of it.

To

To prove this I need only refer to Euler's Memoir on the Origin of Forces among thoſe of the Academy of Berlin for 1750, and the 74th of his Letters to a German Princeſs. He there ſhews that, from a knowledge of the impenetrability of bodies, a ſtranger to the communication of motion muſt infer that a ball in motion cannot paſs through a ball at reſt; therefore a change of ſome kind muſt happen. And from another principle ſtill more general, he muſt infer that the change produced is the ſmalleſt poſſible; a principle from which Maupertuis has deduced all the laws of motion. This principle reſts on this evident ground; that no change is ever greater than the exigency of the circumſtances in which it happens requires; for if it exceeded that exigency, it would, as to ſuch exceſs, be an effect without a cauſe, which we have already ſhewn to be impoſſible. It alſo excludes the other vague ſuppoſitions of Mr. Hume, as Maupertuis ſhews at large in the Memoirs of Berlin for 1746. To be oppoſed by ſuch high authorities is already a ſtrong prejudice againſt him.

In chymiſtry inſtances of inferences of this ſort are ſo numerous that it would be endleſs to recite them. It is indeed in deducing properties before unknown from thoſe already known, that chymical *ſagacity* chiefly conſiſts. The properties inferred were aſſuredly connected with thoſe already known, as without ſuch connexion there could be no inference; juſt as every mathematical propoſition is connected with the propoſitions and axioms that ſerve

ſerve to demonſtrate it; yet as the fact diſcovered precedes the actual experiment, this muſt be called *reaſoning a priori*; and is part of that *ſcrutiny and examination* which Hume aſſerts to be employed in vain. If he ſhould ſay, that the properties from which others are inferred are themſelves known only by experience, I ſhall readily allow it; for as the exiſtence of bodies is not itſelf neceſſary, but reſults from the will of the Creator, ſo neither are the primary laws by which the various ſpecies of bodies and their combinations are governed. But theſe laws being once eſtabliſhed, numerous facts are their neceſſary conſequence, and may thus be traced *a priori*. To expect any other connexion in a ſubject avowedly contingent were abſurd and contradictory.

Section III.

Of the Grounds of Reaſoning from Experience.

Our author now proceeds to ſhew that the credit given to experience itſelf, though, according to him, our only guide, reſts on no ſolid foundation, but is the mere creature of the cuſtom or habit of ſeeing ſome events invariably ſucceeded by others. "When (ſays he, p. 302,) it is aſked, *what is the nature of all* "*our reaſonings concerning matters of fact?* the proper anſwer "ſeems to be, that they are founded on the relation of cauſe "and effect. When again it is aſked, *what is the foundation* "*of*

" *of our conclusions concerning that relation?* it may be replied in " one word *experience*. But if it be asked, *what is the founda-* " *tion of all our conclusions from experience?* this implies a question " of more difficult solution." And, p. 305, " All inferences from " experience suppose as their foundation that the future will " resemble the past, and that similar powers will be conjoined " with similar sensible qualities; but if there be any suspicion " that the course of nature may change, the past may be no " rule for the future, and experience becomes useless." And, p. 307, " When a person has lived so long in the world as to " have observed similar objects or events to be constantly con- " joined together, he immediately infers the existence of the " one from the appearance of the other; yet he has not by all " his experience acquired any idea of the secret power by " which the one object produces the other; nor is it by any " process of reasoning he is induced to draw this inference; still he " finds himself determined to draw it. There is then some other " principle that determines him——this principle is *custom* or " *habit.*"

It may easily be shewn that this principle is not custom and habit, for to constitute these, repeated observations are requisite; whereas the memory of a single instance is often sufficient to induce the mind even of a child, or a brute animal, to expect in similar circumstances an event similar to that already experienced.

Of

Of this Mr. Hume ſeems to have been afterwards ſenſible, for he tells us, p. 314. " that the operation by which we infer like effects " from like cauſes is too eſſential to the ſubſiſtence of mankind " to be left to the ſlow deductions of reaſon; and that it is " more conformable to the ordinary wiſdom of nature to ſecure " ſo neceſſary an act of the mind by ſome *inſtinct*, which may " be infallible in its operation, independently of all deductions " of the underſtanding." And in this I perfectly agree with him; but ſurely it is unreaſonable to pretend that, even on reflection, no other principle can be found for inferring ſimilar effects from ſimilar cauſes in ſimilar circumſtances. A reflecting mind muſt ſoon diſcover that the whole ſyſtem of nature has hitherto been governed by general laws. As long as theſe ſubſiſt, it is evident that events conformable to them may with certainty be expected. The only doubt which can by any poſſibility be entertained, but which in fact never occurs, is whether theſe laws will hereafter exiſt as they have heretofore exiſted. But a ſuſpicion of this kind, ſuppoſing it ever harboured by any man in his ſenſes, is eaſily removed by reflecting on the attributes of the author of nature. As the ceſſation of theſe laws muſt be attended with the deſtruction of the whole ſyſtem, it may rationally be inferred that, whenever that is to happen, it will be announced by preceding ſigns, or otherwiſe; thus that diſaſtrous ſubverſion of the general law to which liquids are ſubjected, in conſequence of which nearly the whole animal

animal race periſhed at the time of the Noachian deluge, was announced by Noah one hundred and twenty years before it happened; and I remarked that every ſucceeding calamity, by which large portions of mankind were affected, and of which diſtinct accounts have reached us, as volcanic eruptions and earthquakes, have conſtantly been announced by extraordinary events, to which if ſufficient attention were paid, the impending deſtruction might have been foreſeen, and in great meaſure prevented; and yet theſe diſaſters are the reſult, not of a violation, but barely of the more occult operation of the laws of nature.

Thus the firſt eruption of Mount Veſuvius in the 79th year of our æra, which overwhelmed Herculaneum and Pompeii, was preceded by earthquakes, and hoarſe rumbling ſubterraneous ſounds reſembling thunder, and an unuſual cloud of ſmoke*.

Sir William Hamilton, relating the late tremendous eruption of this mountain in 1795, remarks that the claſſical accounts of the eruption in 79, and that of the ſubſequent great eruption in 1631, ſo nearly agree, that the former might, by only changing the date, paſs for an account of the laſt, ſo exactly did their circumſtances reſemble each other. And moreover adds, that if the ſubterraneous noiſes, the decreaſe of water in the wells, and a few

* 6 Collect. Acad. and Pliny's Letters.

few puffs of ſmoke obſerved near torre del Græco *two days before*, had been communicated at the time, it might eaſily be foreſeen that an eruption was near at hand, and that its force was directed towards that part of the mountain *.

The earthquake which ingulphed Antioch, in the 115th year of our æra, was preceded by violent ſtorms, thunder and lightning, and an exceſſive heat †.

In 1456 an earthquake ſhook the whole kingdom of Naples, overturned many cities, and deſtroyed 60,000 of the inhabitants; it was preceded by an extraordinary fall of rain, that laſted two months without any wind ‡.

The earthquake which deſtroyed ſeveral cities in Sicily in 163° was announced by ſubterraneous thunders, an uncommon agitation and elevation of the ſurrounding ſea, and violent ſtorms ‖. And that which overturned Catanea and 60,000 of its inhabitants in 1693, was announced by another leſs conſiderable, unuſual clouds and heat the preceding day §.

Lastly, previous to the great earthquake to which Liſbon owed its ſubverſion in 1755, an extraordinary drought prevailed for ſeveral years, and the preceding day an uncommon offuſcation of the ſun **.

Hence

* Phil. Tranſ. 1795, p. 74 and 76. † 6 Collect. Acad. 498. ‡ Ibid. 535.
‖ Ibib. 563. § 2 Phil. Tranſ. Abr. 401. ** Phil. Tranſ. 1755, p. 403, 410.

Hence in the early ages of the world, before the equilibrium of the conſtituent maſſes of the globe had been fully eſtabliſhed, and while, conſequently, internal commotions muſt have been much more frequent, it having been obſerved that extraordinary convulſions were announced by extraordinary appearances, the greateſt attention was paid to ſuch appearances; an attention which at laſt degenerated through fraud and impoſture into the groſſeſt ſuperſtition. Thus judicial aſtrology, ſoothſaying, augury, &c. originated. But the rejection of ſuch abuſes ſhould not prevent attention to uncommon appearances, connected either with the atmoſphere or the earth. By wiſely taking warning from them, only fifteen lives were loſt in the great earthquake which overturned Quito in Peru on the 27th of April 1755, 6 Collect. Acad. 627. In the paſſages already quoted Mr. Hume aſſerts, that by re-iterated experience we gain no idea of the ſecret power by which one object produces another, and thus inſinuates that we have no knowledge of any power: but though ſtrictly ſpeaking we have no idea of *power*, the word *idea* properly denoting only the faint copy of a previous ſenſible impreſſion, yet we have a *notion* of it, ſince we underſtand and can define what we mean by it, namely, that property of an object with which the ſubſtantial or modal exiſtence of another object, not yet in being, is neceſſarily connected and on which it depends; as it is on this property the efficiency of cauſes is grounded, we are certain of its exiſtence, ſince they exiſt, though its eſſence be unknown. Juſt as we are

convinced of the thinking principle within us, of which we alſo have only a notion and not an idea. Mr. Hume need not ſearch for its ſource, for it evidently ariſes from reflection, an operation very different from conſciouſneſs, with which he confounds it in arguing againſt this origin, p. 319, &c.

Section IV.

Of the Nature of Belief.

Mr. Hume's opinion reſpecting the nature of *belief* is as ſingular as any of the opinions we have already examined, and though of much leſs importance than thoſe, yet it may not be amiſs to take ſome notice of it; premiſing that by belief he does not mean the mere perſuaſion of any truth upon the credit of teſtimony, and of which we have no perſonal knowledge, but the perſuaſion that any object whatſoever will be followed by any other as its effect.

To explain the nature of belief, he tells us, p. 310, " that having found in many inſtances that any two kinds of objects, as " flame and heat, ſnow and cold, have always been conjoined together, if either be preſented anew to our ſenſes, the mind is " carried by cuſtom to expect heat or cold, and to *believe* that ſuch " a quality does exiſt, and will diſcover itſelf on a nearer approach." " This belief," he adds, " conſiſts in ſome *ſentiment* or " *feeling*,

" *feeling*, which depends not on the will, nor can be commanded " at pleaſure." Here he confounds the inſtinctive impulſe by which we are induced to judge that theſe objects will be conjoined, or that one ſenſation will be followed by another, with the judgment that ariſes from that impulſe, and which alſo accompanies it, and may be called a ſort of ſentiment or feeling. By reaſon of its accompanying the judgment, both are, when they reſpect a *future* event, denoted by one word, namely, *expectation*. But with regard to *paſt* events, the ſentiment is particularly attended to as accompanying the judgment, and if *ſteady*, it is called *confidence* or *aſſurance*, and both it and the judgment taken together are called *certainty*; and if *unſteady* and wavering in the higheſt degree, they are called *doubt* or *heſitation*; or if the fluctuation be leſs conſiderable, *probability*. Our author, however, thinks that this ſentiment comprehends the whole of what we call *belief*. To define it he allows to be a difficult, if not an impoſſible taſk, but thinks it not improper to attempt a deſcription of it, evidently for the purpoſe of ſhewing that belief is not grounded on reaſon, but a mere blind inſtinct. " Belief (he ſays) is " nothing but a more vivid *conception* of an object than what " the imagination alone is ever able to attain:" forgetting *that* to be the very deſcription he had already given of a *ſenſation* or *impreſſion*, p. 289, where he tells us " there is a conſidera- " ble difference between the perceptions of the mind, when a " man feels the pain of heat, and afterwards recals to his me-

" mory

" mory this ſenſation, or anticipates it by his imagination; theſe " faculties may copy the perceptions of the ſenſes, but never can " reach entirely the force and vivacity of the original ſentiment;" and p. 290, " by the term *impreſſion*, I mean *all* our more lively " impreſſions, when we hear, ſee, or feel, or love, or hate, or " deſire, or will." Now the conception of an object is not an emotion like love or hate, but muſt be either what he calls an impreſſion of ſenſation, or an idea; and he expreſsly tells us that the conception, which he calls belief, is ſtronger and more vigorous than any idea, therefore it muſt be a ſenſation, and this it evidently is not; his account is therefore faulty.

Add to this that belief neceſſarily implies the conjunction of two objects, and therefore cannot be confined to the conception of one only; of this our author himſelf ſeems to have had a glimpſe, for p. 311, he tells us, " that belief is *ſome-* " *what felt by the mind*, which diſtinguiſhes ideas of judgment " from the fictions of imagination." He felt then that judgment was ſomehow involved in belief; but an idea of judgment is an expreſſion both inconſiſtent with his own definition of ideas, and in this caſe appears to me unintelligible.

Section

Section V.

Of Miracles.

After the extraordinary efforts which we have ſeen in the preceding ſections made by Mr. Hume, to ſubvert the rational foundation of the belief of any paſt fact, namely, its conformity to general and uniform experience of ſimilar facts in ſimilar circumſtances, and to attribute the acknowledged influence of ſuch experience on mankind, ſolely to cuſtom and inſtinct, principles which he elſewhere declares to be fallacious, (p. 371.) we ſhould little expect to ſee ſuch conformity ſet up as an evidence of the exiſtence, or the want of it, or oppoſition to it, conſidered as ſuch an evidence of the non-exiſtence of any fact, as to admit of no exception or modification in any poſſible caſe, but, on the contrary, ſuch as ſhould neceſſarily be received as a full and entire proof of either reſpectively. This tergiverſation and complete abandonment of principle we ſhall clearly diſcern in examining his celebrated Eſſay on Miracles; an eſſay which he valued above all others, and to which he told me twenty-two anſwers had been made, hinting, that if any of them had been ſatisfactory any other would have been judged ſuperfluous.

The number of anſwers, after peruſing ſome of them, and particularly that of Dr. Campbell, and that lately given by an ingenious member of our Academy, ſuggeſted to me a concluſion

cluſion very different from that deduced by Mr. Hume. It appeared to me that the arguments advanced in that eſſay were incompatible with ſo many uncontrovertible truths, that their falſehood might be variouſly demonſtrated by various perſons; and as mathematical problems daily receive various ſolutions without implying the inſufficiency of the foregoing, ſo the artificial diſguiſes of error may be variouſly detected by ſeveral, and with equal ſucceſs. Emboldened by theſe conſiderations I alſo venture on this field of diſputation, on which I engage our common adverſary in a method ſomewhat different from that followed by others, namely, by purſuing him ſtep by ſtep.

THIS mode of refutation, though apparently the moſt direct and appoſite, is however, ſometimes expoſed to this inconvenience, that when general ſources of fallacy or error are widely ſcattered through a declamatory performance, ſuch as that I now conſider, it is not eaſy to ſelect the particular paſſages beſt ſuited for their detection. This difficulty I mean to obviate by ſtating the moſt prominent of each kind in preliminary remarks.

1°. A CONSTANT fallacy lurks in our author's application of the term *experience*; ſometimes he applies it to our own paſt or actual experience of which we have a metaphyſical certainty; ſometimes to that of others only, and not our own,

of

of which we can have at moſt only a moral certainty; and ſometimes he denotes by it a mere conformity to paſt experience, *either* of our own or of others, which is often attended with phyſical or moral certainty, and often with bare probability: theſe different ſenſes he dexterouſly ſhifts and employs as beſt ſuits his purpoſe.

2°. The radical error that pervades the whole of this Eſſay, and is indeed the corner ſtone on which his whole theory muſt reſt, even if the equivocal uſe of the term *experience* had been avoided, conſiſts in aſcribing the ſame immutability to the laws by which corporeal nature is governed, as to thoſe which are inherent in the nature of moral agents. Knowledge of the former is conveyed to us chiefly by experience; that of the latter ariſes partly from experience, but being homogeneous with, and analogous to our own feelings, partly alſo from conſciouſneſs: the former are clearly diſcerned to proceed from the power and wiſdom of the author of nature, which experience itſelf ſhews us not to require their abſolute immutability in all poſſible circumſtances. Thus no law has ever been conſidered leſs mutable than that of the deſcent of bodies when unſupported, yet exceptions to it have at laſt occurred, not only through the now well known, but hitherto inexplicable, powers of magnetiſm and electricity, but alſo in the adherence of the hardeſt poliſhed

bodies to each other; and to what degree, on what occaſions, and in what circumſtances the moſt general laws of nature may ſtill be found to vary, or to have varied, we are profoundly ignorant. But with regard to the laws that originate in the nature, and are eſſential to the conſtitution of rational agents, particularly of the human kind, the caſe is very different; though they alſo often reſtrict, qualify, or modify each other to a ſurpriſing degree, yet the extent, to which, in conſequence of theſe modifications, the apparent anomalies of human conduct can reach as long as men retain the uſe of their reaſon, is perfectly known,* and aberration beyond this limit being inconſiſtent with rational nature muſt be deemed impoſſible.

If therefore the laws of phyſical and thoſe of moral nature be in any caſe ſo oppoſed to each other, that both cannot be reconciled, but one or other muſt be deemed to have been infringed, it is eaſy to diſcover which of them, the one being abſolutely, the others only hypothetically inviolable, namely, in certain known circumſtances.

I now

* Nous qui ſommes hommes, ne ſçavons nous pas bien *juſqu' a quel point* d'autres hommes ont pu être ou impoſteurs ou dupes?

Fontenelle's Hiſt. des Oracles, p. 12, in 8vo.

I NOW proceed to the examination of the eſſay.

Page 345. IN the firſt paragraph an inaccurate expreſſion occurs, incompatible with the philoſophic preciſion to be expected in argumentative treatiſes. He tells us " that a weaker evidence " can never deſtroy a ſtronger." Now all evidence is equally ſtrong, and a weak evidence is no evidence. It is only in a forenſic ſenſe, where evidence is taken for ſynonimous with proof, that this poſition can be allowed to be true. In the philoſophic ſenſe one evidence may be more eaſily and immediately diſcerned than another, but when diſcerned it is equally ſtrong. Thence he infers, that " the evidence of the truth " of the chriſtian religion is leſs than the evidence of the truth " of our ſenſes;" which is alſo a miſtake. The evidence of our ſenſes, being immediate, is more eaſily attained than that of the truth of chriſtianity, which reſults from a comprehenſive view of the numerous arguments that produce it; but this evidence, when once attained, is equal to that of our ſenſes. Thus the evidence reſulting from the complex demonſtrations of Apollonius or Archimedes is as ſtrong as that of the primary axioms of geometry, though much more difficultly attained; the evidence of tranſmitted teſtimony is frequently as ſtrong, and as juſtly excludes all doubt, as the evidence of our ſenſes. Can any one now doubt that ſuch perſons exiſted, as William the III, Henry the VIII, or even Julius Cæſar, or Alexander, &c. or of the exiſtence of

Rome, Conſtantinople, or Jeruſalem, any more than if he had ſeen them?

P. 344. "SOME events have been found in all countries, "and in all ages, to have been conſtantly conjoined together, "others are found to have been more variable; ſo that in our "reaſonings concerning matters of fact, there are all imaginable "degrees of aſſurance, from the higheſt certainty to the loweſt "ſpecies of moral evidence." Here Mr. Hume, in common with many others, confounds certainty with evidence; yet they are perceptions perfectly diſtinct. Evidence is a clear diſcernment, certainty a full aſſurance of truth. Evidence is always accompanied with certainty, but certainty is often deſtitute of evidence. Thus ſome Mahometans are as certain of the truth of their religion as we are of ours, to ſay nothing of our own various ſects, yet none will ſay that both are founded on evidence. Evidence ariſes from intuitive, demonſtrative, or inſtinctive knowledge; certainty often from early habits, a partial view of a ſubject, or a paſſionate regard for ſome ſentiment or opinion. Evidence excludes all poſſible doubt, certainty only all actual doubt. Evidence is contraſted with obſcurity, certainty with probability or doubt. We are certain that future events of a phyſical nature will correſpond with thoſe that we have heretofore conſtantly and uniformly experienced, but we have no intrinſic evidence of ſuch correſpondence and conformity in all caſes; for as Hume juſtly obſerves, the contrary involves no contradiction.

AND

And here for the ſake of perſpicuity it may be proper to notice a few general circumſtances in which this aſſurance ariſes, and which ſerve to diſtinguiſh it into diſtinct ſpecies.

The firſt is *metaphyſical* certainty, which is commonly attended with, and grounded upon, evidence. This attends our actual ſenſations and other perceptions, and the clear and diſtinct memory of the paſt, and all demonſtrative reaſonings deduced from our perceptions, &c.

The ſecond is *phyſical* certainty, which is ſeldom attended with evidence; of this nature is the aſſurance that attends the belief or expectation of any future phyſical fact.

The third is moral certainty, which attends ſelf-evident or demonſtrative moral truths, and frequently the belief of the paſt or future actions of rational agents. It is often grounded upon, but often alſo unaccompanied by, evidence.

The fourth is of a *mixed* nature, regarding paſt or diſtant phyſical facts, which we have never experienced, and is grounded partly on their conformity with our own experience, and partly on teſtimony. Sometimes the one, and ſometimes the other, is the predominant ingredient that produces this certainty. I now return to Mr. Hume.

Page 344. " A wiſe man therefore proportions his belief to " his evidence" (he means to his proof): " in ſuch concluſions as " are founded on an infallible experience, he expects the event " with the laſt degree of aſſurance, and regards his paſt experi- " ence as a full proof of the future exiſtence of that event." Let us here conſider what experience he dignifies with the appellation of *infallible*. It ſurely is not his own *perſonal* experience, but that general and uniform experience of others which he could know only by teſtimony, for if he never before fired a piſtol or a cannon, he would, on attempting it, expect the event experienced by others, with the ſame degree of aſſurance, as if gained by his own practice; but neither his nor their experience can be denominated *infallible*, for both engines often miſs fire or burſt. This term indeed is never applied to experience, but by quacks or mountebanks, who promiſe infallible cures, &c. The expectation of *wiſe men* is generally confinedto a high degree of probability, and ſeldom riſes to certainty, except in very ſimple caſes; and even then abſolute infallibility on all poſſible occaſions, and in all poſſible circumſtances, is never aſcribed to it.

Ibid. " In all caſes of probability we muſt balance the op- " poſite experiments, and deduct the ſmaller number from the " greater, in order to know the exact force of the ſuperior evidence," " or rather probability—This mode of eſtimating probabilities is

is erroneous, and in many cases productive of absurdity. Mathematicians, who have treated this subject with the most rigorous accuracy, follow a very different method. They denote certainty by an unit, and probability by fractions of which the denominator consists of the number of times or trials in which the experiment has either happened or failed, and the numerator of one of them exhibits the number of trials in which it has happened, and that of the other the number of trials in which it has failed; and then they consider their *ratios*, and not their difference. Thus if an event has happened ten times, and has failed five times, the probability that it will happen again is $\frac{10}{15}$, and the probability that it will not happen again is $\frac{5}{15}$: then its probability is as two to one. Whereas by Mr. Hume's rule of subtraction, the superior probability would be only $\frac{5}{15}$, and thus would be converted into an improbability; for an experiment that has succeeded in half the number of trials to which it had been exposed, is justly considered as doubtful, it having failed as often as it succeeded: but if it has not succeeded even in half the number of trials, it must be considered as *improbable*.

Ibid. "There is no species of reasoning more useful or necessary to human life, than that derived from the testimony of spectators. We may even deny it to be founded on the relation of cause and effect; but it is sufficient to observe, that our assurance in any argument of this kind is solely derived from

"our

" our obſervation of the veracity of human teſtimony, and of the " uſual conformity of facts to the reports of witneſſes, which, like " all other objects, have no diſcoverable connexion; nor can any " inference be drawn from one to the other, but ſuch as is " founded merely on experience of their conſtant and regular " conjunction." This Dr. Campbell and others deny, and I believe juſtly; for credit is originally given to teſtimony, and even to experience itſelf, from inſtinct, and in moſt caſes by perſons who never reflected on the motives mentioned by our author. Experience barely teaches us to reſtrict the confidence we are by nature prompted to repoſe in teſtimony indiſcriminately, and confine it to teſtimony duly circumſtanced, as Dr. Campbell has alſo well obſerved. But though I admit this principle, I ſhall not avail myſelf of it in the ſequel of this controverſy.

Ibid. " As the evidence derived from human teſtimony is " founded on paſt experience, ſo it varies with that experience, " and is regarded as a *proof*, or a probability, according as the con- " junction between any particular kind of report and any kind of " objects has been found to be conſtant, or variable."

This argument is a palpable ſophiſm, founded on the double meaning of the term experience; it denotes both perſonal experience, and the experience of others; let it be confined to either ſenſe ſingly, and it will be found falſe in the firſt, and abſurd in the

the ſecond. For firſt, the evidence of teſtimony cannot be ſaid to be founded on our perſonal paſt experience of the conſtant, or variable, conjunction of reports and their objects; for we juſtly give credit to numerous reports, of whoſe conjunction with their objects we never had, nor could have, any perſonal experience, nay not even of any thing analogous to them. Did not Mr. Hume give credit to the reports of volcanos? Did he ever ſee them, or any thing like them? Did he not believe the decapitation of Charles the firſt, as we do that alſo of Lewis the ſixteenth, though we neither ſaw it, nor any thing analogous to it? Did he ever ſee a murder of any kind committed? Yet did he not believe the reports of ſuch crimes? But it were endleſs and ſuperfluous to enlarge on this head. In this ſenſe of the word experience his argument is evidently falſe. I therefore paſs to the ſecond ſenſe. In the ſecond ſenſe the argument would run thus: *the evidence derived from teſtimony is founded merely on the experience of other men.* This ſenſe is too glaringly abſurd to need being inſiſted upon; for how can I know the experience of others, but by teſtimony? And, this being granted, the argument would amount to this: *the evidence derived from human teſtimony is founded on human teſtimony.*

Neither can it be ſaid that the word *experience* is taken in a compound ſenſe, denoting partly our own perſonal experience, and partly that of others; for though this be true in a certain ſenſe, it is not ſo in that which Mr. Hume's argument requires,

namely, that they ſhould reſpect the ſame object; for they are too often ſeparated to be true in this ſenſe. His argument is therefore falſe in every ſenſe he could aſcribe to it. The true foundation of the credibility of human teſtimony, in caſes not miraculous, is that laid down by all philoſophers and juriſts, namely, the conſiſtency of the fact atteſted with the known laws of nature, and ſuch qualifications and circumſtances of the witneſſes as we know both from reaſon * and experience ſhould entitle them to credit.

P. 345. " Many particulars diminiſh or deſtroy the force of any " argument derived from human teſtimony; for inſtance, if the " fact partakes of the *extraordinary* and the *marvellous*, in that caſe " the evidence reſulting from the teſtimony admits a diminution " greater or leſſer in proportion as the fact is more or leſs unuſual. " For the reaſon *we* place any credit in witneſſes or hiſtorians, is " not from any connexion which we perceive *a priori* between " teſtimony and reality, but becauſe *we* are accuſtomed to find a " conformity between them; but when the fact is ſuch as ſeldom " falls under *our* obſervation, there is a conteſt of two oppoſite ex- " periences, of which the one deſtroys the other as far as it goes. " The ſame principle of experience which gives *us* a certain degree " of aſſurance in the teſtimony of witneſſes, gives *us* alſo in this " caſe

* Note, *Reaſon* here denotes the knowledge derived partly from conſciouſneſs, and partly from that circuitous experience well noticed and explained by our author in the note p. 308.

" caſe another degree of aſſurance againſt the fact which they en-
" deavour to eſtabliſh; from which contradiction there neceſſarily
" ariſe a counterpoiſe and mutual deſtruction of belief and au-
" thority."

In this paragraph many particulars indeed deſerve animadverſion.

1º. The extraordinary and the marvellous ſhould not be confounded. Many facts are extraordinary, yet not marvellous; and many marvellous, that are not extraordinary. The birth of twins is not an ordinary fact, nor is the death of a man at the age of one hundred years, yet, though there be but one inſtance of the firſt in a flock of one thouſand ewes, and of the other in a mortuary liſt of ten thouſand deaths, a common ſhepherd is credited in the firſt caſe, and an unknown compiler of a bill of mortality in the ſecond caſe, without any diminution of credit; and the reaſon is, becauſe neither fact is contradictory to the known laws of nature, though both are unuſual, and the laws relative either to the origin or ceſſation of life are in great meaſure unknown, and becauſe ſuch facts, though by far not the moſt uſual, are yet known by teſtimony to have often occurred.

2º. It is plain from the conſtant recurrence of the words *we* and *us*, that by the experience mentioned in this paragraph our own perſonal experience is denoted, and indeed the tenor of

his argument requires it to be taken in that ſtrict confined ſenſe; and yet in this ſenſe his aſſertion is plainly falſe. In innumerable inſtances we give credit to teſtimony concerning facts, whereof, not only we have not been accuſtomed, but alſo have never obſerved the conformity with the teſtimony. Thus, to ſay nothing of the relations of earthquakes and volcanos, the correſpondence of which relations with the facts deſcribed could not be known to Mr. Hume from his own experience, as he never obſerved either of thoſe phænomena, I would aſk him, when had he or any of his acquaintance ſeen a king put to death after a trial, by his own ſubjects? A fact ſurely both marvellous and extraordinary, of which there are but two or three inſtances in the annals of mankind, and yet he believed, and is himſelf the hiſtorian of ſuch a fact.

3°. He tells us that with regard to *ſuch* facts, "the ſame "principle of experience which gives us a certain degree of "aſſurance in the teſtimony of witneſſes, gives us in this caſe "another degree of aſſurance againſt the fact; and hence a contra-"diction, from which there neceſſarily ariſes a counterpoiſe and "mutual deſtruction of belief and authority." Let him apply this reaſoning to the caſes juſt mentioned, and he will perceive its illuſion.—But to examine it more ſtrictly, let us remark that it ſuppoſes, that of one and the *ſame* numerical fact our experience gives us an aſſurance which is contradicted by teſtimony; for if only a *ſimilar* fact be meant, there can be no contradiction, no more than if

if he had obſerved that on the firſt of June 1770 it had rained in Edinburgh, and a witneſs depoſed that it did not rain on that day at London, nor in Edinburgh on the firſt of June 1771. But if he means the ſame numerical fact, there may indeed be a contradiction, but there will be no counterpoiſe nor mutual deſtruction of belief and authority, for he aſſuredly will adhere to his own experience, and hold the contradictory teſtimony at nought.

I SHALL take no notice of the proverbial ſaying at Rome relative to Cato, as every one knows ſuch trite ſayings are looſe modes of expreſſion, and never ſtrictly and literally taken.

P. 345. "THE Indian Prince, who refuſed to believe the firſt "relation concerning the effects of froſt (in Holland), reaſoned "juſtly. It naturally required very ſtrong teſtimony to engage "his aſſent to facts, which aroſe from a ſtate of nature with which "he was not acquainted, and bore ſo little analogy to thoſe events "of which he had conſtant and uniform experience:—though they "were not contrary to his experience, they were not conformable "to it."

ACCORDING to Mr. Hume's principles, certainly this Indian prince (the king of Siam) reaſoned very juſtly, and ſhould not admit that water was converted into ice by any degree of cold, upon any teſtimony; nor does it appear that he ever did admit it.

Neither

Neither ſhould he admit that there exiſted any ſtate of nature which he had not himſelf obſerved, and which was contradicted by the general and uniform experience of the kingdom of Siam. Mr. Hume indeed ſays, it was not contrary to his experience, but merely not conformable to it;* but this non-conformity is in fact a contrariety, ſince liquidity and ſolidity are contrary ſtates. Nay, according to Mr. Hume's mode of reaſoning in the preceding paragraph, it was even a contradiction; for *his* experience informed him that water was *always* liquid, and the fact *related to him* was that water was *ſometimes* ſolid (no allowance being made for different climates): neither ſhould he believe that there exiſted, in any ſeaſon, a degree of cold which he had never experienced. But in reality this Indian Prince reaſoned very abſurdly; for he muſt have known that lead, ſilver and gold, or at leaſt that pitch, wax and tallow are rendered liquid by a certain degree of heat and become ſolid when cooled: by analogy he ſhould therefore conclude, that water might ſimilarly be affected by greater degrees of cold; and that it was ſo, he ſhould have admitted on proper teſtimony, as all Europe did upon Gmelin's teſtimony, that mercury was frozen in Siberia, long before the illuſtrious Cavendiſh had repeated the experiment in England. He ſhould alſo have known that ice was not unfrequent in the northern parts of Ava and the mountains of Cochinchina, kingdoms bordering on Siam.

Mr.

* Locke alſo calls it contrary: book iv. chap. 15, § 5.

Mr. Hume however, conſiſtently enough I muſt own, endeavours to juſtify the incredulity of the Indian Prince on this occaſion; for, p. 346, ſays he, "It muſt be confeſſed, that in the caſe "of freezing the event follows contrary to the rules of analogy, "and is ſuch as a rational Indian could not look for. The opera- "tions of cold upon water are not gradual according to the de- "grees of cold, but whenever it comes to the freezing point, the "water paſſes in a moment from the utmoſt liquidity to perfect "hardneſs. Such an event may therefore be denominated *extra-* "*ordinary*, and requires a pretty ſtrong teſtimony to render it "credible to people in a warm climate."

This ſtatement is incorrect in ſeveral points. I have already ſhewn that the freezing of water is analogous to that of other liquids, whether naturally or artificially rendered ſo, each of which has its peculiar point of conſolidation. And though water (nor other liquids) does not congeal before it comes to its freezing point, yet it may be cooled below that point before it freezes: it does not therefore in a moment paſs into ice when it comes to that point; nay at all times, if its maſs be conſiderable, it muſt be long expoſed to that degree of cold, or to a much greater, before it is converted into ice, and its converſion is gradual, a cruſt being firſt formed on its ſurface, which ſucceſſively acquires both depth and hardneſs, both being in great meaſure proportioned to the degree of cold. In this reſpect its congelation is analogous to that of wax and melted metals.—He adds that, "as they never ſaw water in "Muſcovy

" Muſcovy during the winter, they could not reaſonably be " poſitive what could be the conſequence." If ſo, the fact ſhould appear to them merely doubtful, and not contrary to analogy; conſequently, even a ſlight teſtimony ſhould ſuffice to eſtabliſh it.

Ibid. " But in order to increaſe the probability againſt the " teſtimony of witneſſes, let us ſuppoſe the fact which they " affirm, inſtead of being only marvellous, is really miraculous; " and ſuppoſe their teſtimony, conſidered apart, amounts to an " entire proof, in that caſe there is proof againſt proof, of " which the ſtrongeſt muſt prevail, but with a diminution of " its force in proportion to that of its antagoniſt."

Here is a glaring inaccuracy; for by *full proof* Mr. Hume underſtands ſuch a proof as produces the laſt degree of aſſurance, and by an *entire proof* he evidently means a proof equally ſtrong; how then can he ſuppoſe, in any caſe, one ſtronger than the other, and that the ſtrongeſt ſhould prevail?

Ibid. " A miracle is a violation of the laws of nature; and as a " firm and unaltered experience has eſtabliſhed theſe laws, the " proof againſt a miracle from the very nature of the fact is as " entire as any argument from experience can poſſibly be " imagined—nor can ſuch a proof be deſtroyed, or the mi-

" racle

" racle rendered credible, but by an oppoſite proof which is " ſuperior."

Here it is ſuppoſed very properly by our author, that the laws of phyſical nature may be contradictorily oppoſed by thoſe of moral nature; for the proof by teſtimony of the violation of the former cannot be ſuppoſed full and entire, much leſs a ſuperior proof, unleſs the falſehood of the teſtimony ſhould imply a violation of the laws by which rational agents are governed. Now as it is metaphyſically evident, that two contradictory propoſitions cannot both be true, but one muſt be true, and the other falſe, ſo one and the ſame fact cannot be ſuppoſed to have exiſted in conformity to the laws of moral nature, and not to have exiſted in conformity with the laws of phyſical nature. It is plain therefore that one or other of theſe laws muſt have been infringed, ſo that a miracle of ſome kind muſt have happened: if we do not admit that of a phyſical nature, we muſt admit that of a moral nature, and *vice verſa*. We are therefore ſeriouſly to conſider, which of theſe laws muſt, conſiſtently with right reaſon, the occaſion, and circumſtances, be deemed to have been infringed; and not content ourſelves with a mere ſuſpenſion of judgment, unleſs the matter no way concerns us. Mr. Hume ſeems indeed to have decided this point; for he tells us, that the laws of phyſical nature are firm and unalterable: how he has learned their inalterability I cannot tell. That they have not been altered, except in a few inſtances

and for a ſhort time, we may conclude from univerſal and nearly an uniform experience. But that the great Being that eſtabliſhed them, could not alſo interrupt their action, or violate them on any occaſion, I ſee no reaſon to ſuppoſe. Nay Mr. Hume himſelf, in a note in the ſecond part of this eſſay, admits the poſſibility of their alteration; for he tells us, "that "the decay, corruption and diſſolution of nature is an event "rendered probable by many analogies." The truth of this aſſertion, which Mr. Hume very unconſequentially admitted on the teſtimony of a few aſtronomers, I need not examine; but I entertain no doubt but they were violated at the time of the univerſal deluge, a fact atteſted, not only by the almoſt unanimous traditions of the moſt antient civilized nations, but by the evident traces of that cataſtrophe ſtill apparent in many parts of the globe. That the phyſical laws of nature have ſince been interrupted on various occaſions, for the eſtabliſhment of the Jewiſh and Chriſtian revelations, if any credit can be given to ſuch teſtimony as Mr. Hume calls a full proof, ſeems to me demonſtrated. A cauſe capable of interrupting theſe laws, and an occaſion worthy of that interpoſition, have been aſſigned, the moſt cogent teſtimony that ſuch interruption has actually taken place is produced, and the connexion of the preſent ſtate of mankind by innumerable links with the belief of ſuch interruption, in every age ſince the commencement of our æra, is ſtill apparent. But a cauſe capable of producing a moral miracle in ſupport of a falſehood, though anxiouſly ſought for by ſome

antient

antient and many modern philoſophers, remains ſtill to be diſcovered.

Page 347. In the ſecond part of this eſſay our author tells us, "he had been much too liberal in ſuppoſing that the teſti-"mony upon which a miracle is founded may poſſibly amount "to an entire proof, ſo that the falſehood of ſuch teſtimony "would be a kind of *prodigy*." What ſignification he attached to the word *prodigy* I cannot determine. In its uſual acceptation it denotes ſomething produced by nature, but out of its common courſe; not a manifeſt violation of its *known* laws, but the reſult of *unknown* laws acting in unknown circumſtances: ſuch the effects of gunpowder, electricity and magnetiſm muſt, when firſt diſcovered, have appeared, or even the elevation of water in a common pump; and ſuch the production of an animal with two heads muſt ſtill appear. But the *moral* laws, or motives which can influence men in their ſenſes, are perfectly known; and hence a line of conduct incompatible with theſe in known circumſtances muſt be deemed abſolutely impoſſible.

Ibid. The circumſtances our author thinks requiſite, to give us a full aſſurance in the teſtimony of men, at leaſt in caſe of miracles, are ſome of them new and extraordinary, being ſuch as no juriſt ever required. "A miracle (he tells us) ſhould, to "give us this aſſurance, be atteſted by a ſufficient number of "men of ſuch unqueſtioned good ſenſe, education, and learning,

" as to ſecure us againſt all deluſion in themſelves." If by good ſenſe he means that they ſhould not be idiots, enthuſiaſts, or madmen, this qualification cannot be diſputed; but why education and learning are required to atteſt *all ſorts* of miraculous events, I cannot conjecture, as the knowledge of many ſuch events requires nothing more than the free uſe of one's ſight or feeling. It is plain that neither education nor learning is requiſite to obſerve, that a man notoriouſly blind from his nativity is made to ſee without any chirurgical operation, or that a man notoriouſly dead is reſtored to life; or to ſee a man walking on water as on firm land, &c. theſe are not tricks or impoſtures that require ſome ſkill for their detection.

The ſecond qualification he requires, is " ſuch undoubted " integrity as to place them beyond all ſuſpicion of any deſign " to deceive others." To this I perfectly agree.

The third is " ſuch credit and reputation in the eyes of " mankind, as to have a great deal to loſe in caſe of being " detected in any falſehood." He might add, *or at leaſt a great deal to fear in caſe of detection.* For men of an inferior claſs may be little known, and therefore not loſe much credit or reputation; but they may well fear corporal puniſhment, loſs of liberty or life.

Ibid. 4thly, He requires " that they ſhould atteſt facts per- " formed in ſuch a public manner, and in ſo celebrated a part " of

" of the world, as to render detection (if falſe) unavoidable." The teſtimony of witneſſes, otherwiſe duly qualified, and acting under the terror of puniſhment, if found to prevaricate, does not appear to me to require much publicity to be intitled to credit: but this depends on various collateral conſiderations; for unleſs the falſehood of the teſtimony were itſelf miraculous, that is out of nature, I ſhould give it no credit in ſuch caſes as we now diſcuſs, whether the event were public or private. Neither do I ſee the neceſſity that the miracle ſhould be performed in a celebrated part of the world; for, if falſe, its falſehood may eaſily be detected, or at leaſt it may eaſily be rendered ſuſpicious by thoſe that are intereſted in diſcrediting it, whether performed in the country, or in a celebrated capital. Nay the ſcene of ſome of the groſſeſt impoſtures of this nature has been laid in celebrated capitals, as Alexandria, Naples, Paris, &c.

After enumerating ſome of the circumſtances that ſhould attend the atteſtation of a miracle, our author paſſes to thoſe that, in his opinion, generally promote its reception and credit: I ſay *ſome*; for the circumſtances that have preceded, accompanied, and ſucceeded the atteſtation of real miracles, are much more numerous and cogent.

Ibid. He tells us, that " though we readily reject any " fact that is unuſual and incredible in an ordinary degree, " yet in advancing farther, the mind obſerves not always " the

" the ſame rule; but when any thing is affirmed utterly
" abſurd and miraculous, it rather the more readily admits
" ſuch a fact, upon account of that very circumſtance which
" ought to deſtroy all its authority. The paſſion of ſur-
" priſe and wonder, ariſing from miracles, being an agreeable
" emotion, gives a ſenſible tendency towards the belief of thoſe
" events from which it is derived."

That ſuch a retrogreſſive diſpoſition reſembling a curve of double inflexion, rejecting a fact marvellous in a low degree, and readily receiving one marvellous in the higheſt degree, ſhould exiſt in the human mind, would indeed be an extraordinary phænomenon, and ſuch as I am not diſpoſed to admit on the teſtimony of Mr. Hume; it being contrary to my own obſervation, though I have lived ſomewhat longer than he has. The general belief of ſecond ſight among the vulgar of his country probably ſuggeſted this notion to him. He ſhould however remark, that the ready admiſſion of the marvellous never takes place, even among the weakeſt men, and the moſt ignorant vulgar, except with regard to objects coincident with, or at leaſt not repugnant to their religious belief, ſuch as ſtories of witchcraft, apparitions, magick, &c. and even here that retrograde diſpoſition does not appear, for the credulity of the moſt credulous has its bounds. Nay all will certainly reſiſt this paſſion for the marvellous, if it be oppoſed by tendencies much more powerful, as attachment to their rooted prejudices, their fixed

fixed habits, and regard to their immediate intereſts. A teſtimony oppoſed by ſuch obſtacles will certainly never be received through a mere love of the marvellous. The inſtances our author adduces of wonderful tales believed on the credit of travellers, or various hiſtorians, prove nothing more than the exiſtence of a love of the marvellous in the human breaſt, which I readily admit. But the circumſtances attending ſuch looſe tales, and thoſe that have attended the teſtimony of real miracles, are ſo ſtrikingly different, that none but the moſt ignorant can require a detailed compariſon.

The next circumſtance, which, according to Mr. Hume, promotes the credit of ſupernatural and miraculous relations, "but "in reality forms a very ſtrong preſumption againſt them, is, "that they are obſerved chiefly to abound among ignorant and "barbarous nations; or, if a civilized people has ever given "admiſſion to any of them, that people will be found to have "received them from ignorant and barbarous anceſtors, who "tranſmitted them with that inviolable ſanction and authority, "which always attend antient and received opinions." Preſumptions are certainly of weight, where proofs from the nature of the caſe cannot be expected. *Stabit præſumptio donec probetur contrarium*, is the rule of law; but where proofs can be reſorted to, preſumptions are of little weight. Prodigies, omens, and oracles are, it is true, frequently mentioned by pagan hiſtorians, as having occured in ages of barbariſm and ignorance; but ſo far are they from having been tranſmitted with that inviolable

ſanction

ſanction and authority which our author mentions, that the hiſtorians themſelves frequently relate them as doubtful. Miracles on the contrary are ſcarce ever mentioned by pagan hiſtorians. It is in countries perfectly civilized, and in the moſt enlightened periods, that theſe are ſaid to have been performed. Let them happen where they may, it is the circumſtances, occaſion, and teſtimony that ſupport them, which muſt finally evince their reality.

Our author adds, "a judicious reader is apt to ſay upon the "peruſal of theſe wonderful hiſtorians, ſtrange that ſuch pro- "digious events never happen in our days! but it is nothing "ſtrange I hope, that men ſhould lie in all ages." Nor is it more ſtrange, that preſumptuous ignorance ſhould in all ages deny the moſt certain facts. Witneſs the king of Siam already mentioned. Many men of education denied, as I well remember, the poſſibility of the elevation of balloons for ſeveral months after their invention. Deſcartes denied the poſſibility of Archimedes's burning mirrors, ſince verified by Buffon. The teleſcopic diſcoveries of Galileo were looked upon by ſome philoſophers as idle dreams, *fitter ſubjects of deriſion than of reaſoning.* Lord Bacon, as Hume himſelf obſerves, gave no credit to the Copernican ſyſtem. Many prodigies mentioned by Livy, ſuch as monſtrous births, fire appearing on the points of ſpears, have been alſo derided; though abundant proofs of ſuch births may be ſeen in the volumes of the tranſactions of many Academies, and fire or lambent flame

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on the points of ſpears in certain ſituations is now well known to be no uncommon electrical phænomenon.*

Here our author concludes his general reaſoning againſt the credit of miracles upon any teſtimony; and as the remainder of his eſſay is nothing more than an attempt to prove, that many acknowledged impoſtures are as credible as thoſe miracles whoſe credit is moſt firmly eſtabliſhed, a ſubject foreign to academic diſcuſſion, I ſhall here cloſe my obſervations on the paradoxes he has advanced.

* Pilorum cacumina ſua ſponte arſerunt. Cæſar de bello Africano cap. 6°. Plin. Hiſt. Nat. lib. 2. cap. 37. Prieſtley's Hiſt. of Electricity, p. 395.

1800.	Higheſt	IN. Inches.	STORMS.
January - -	30,24	3,980384	5, S. to E. W. & S W.
February - -	30,50	1,261112	
March - -	30,54	1,103473	
April - -	30,16	3,310419	5, E. W. S W. & N W.
May - -	30,50	1,812849	2, E. W. Thun. & Light.
June - -	30,50	0,788195	
July - -	30,66	0,630556	
Auguſt - -	30,60	0,472917	
September -	30,47	2,246355	2, N W.
October - -	30,66	2,995141	5, N W. E. & W. by S.
November -	30,60	1,615799	4, W. by S. W. & N W.
December - -	30,30	3,349828	1, W. by S.
Mean of Year -	30,478	23,567028, Total of the Year.	24

Synoptical View of the State of the Weather at Dublin in the Year 1800.

By RICHARD KIRWAN, Esq. LL. D. P. R. I. A. and F. R. S.

1800.	BAROMETER.					THERMOMETER.			RAIN.		STORMS.
	Higheſt.	Day it happened.	Loweſt.	Day it happened.	Mean of the Month.	Higheſt in the Day.	Loweſt at Night.	Mean of the Month.	Days.	Inches.	
January	30,24	1ſt S. and S E.	29,16	23d S W.	29,56	49	26	38,28	17, on 2 of which fell Snow	3,980384	5, S. to E. W. & S W.
February	30,50	4th W. by N.	29,42	23d E.	29,69	49	30	38,57	13, on 1 of which fell a little Snow	1,261112	
March	30,54	20th & 21ſt N E. & N W.	29,48	31ſt W. S. and N.	30,03	52,50	28	41,19	14, on 1 of which fell a little Snow	1,103473	
April	30,16	27th E.	29,24	16th W. by S.	29,75	58	34,50	45,65	25, on 1 of which fell a little Snow	3,310419	5, E. W. S W. & N W.
May	30,50	28th E. to S.	29,20	17th N W.	30,02	62	39	50,49	20, on 2 of which fell a little Hail	1,812843	2, E. W. Thun. & Light.
June	30,50	27th and 30th W. and N.	29,94	21ſt W.	30,28	73	38	55,35	12, on 2 of which fell ſome Hail	0,788195	
July	30,66	24th and 25th E.	29,90	7th S W.	30,40	79	46	61,41	11	0,630556	
Auguſt	30,60	9th E.	30,10	19 to 22d E. N. & N W.	30,32	81,50	44	59,87	10	0,472917	
September	30,47	13th E.	29,33	21ſt E. by S.	29,98	70	41,50	55,03	21	2,246355	2, N W.
October	30,60	16th W. by N.	29,17	9th W. by S.	30,08	60	35,50	47,65	19	2,995141	5, N W. E. & W. by S.
November	30,60	21ſt N.	29,07	8th var. E. S. and N W.	29,84	55	26	41,27	14, on 1 of which fell a little Snow	1,615799	4, W. by S. W. & N W.
December	30,30	16th E.	29,22	4th N.	29,79	54	23	39,02	21, on 4 of which fell Snow	3,349828	1, W. by S.
Mean of Year	30,478		29,436		29,978	61,91	34,30	47,819	197, on 10 of which fell ſome Snow	23,567128. Total of the Year.	24

OBSERVATIONS ON CALP.

By the Honourable GEORGE KNOX, *M. R. I. A.*

Read, March 9, 1801.

AS the Academy has evinced a deſire to promote mineralogical enquiries in the county of Dublin; I take the liberty of ſubmitting to it the analyſis of a mineral which abounds in the neighbourhood of the metropolis, and which our celebrated Preſident has diſtinguiſhed by the name of Calp. (vid. Kirwan's Elements of Mineralogy, vol. I. p. 233.)

CALP, or black quarry ſtone of Dublin, is placed, in Mr. Kirwan's Elements of Mineralogy, under the argillaceous genus; being a ſubſtance which poſſeſſes the diſtinctive characters of that earth more than any other. For, although it effervesces with acids and ſcratches glaſs, it neither burns to lime nor gives fire with ſteel. Whilſt on the other hand it emits, when breathed upon, the ſmell peculiar to argillaceous earth.

THE quantity of argill, however, which its chemical analyſis diſcovers, would ſcarcely, were its internal properties to determine its claſs, entitle it to rank in that which Mr. Kirwan has aſſigned to it.

The external characters and leading properties of the ſpecimen which I uſed were the following:

Colour—Greyiſh black, inclining to blue.

Lustre—o

Transparency—o

Hardness—Scratched glaſs with difficulty, crumbling at the ſame time.—Did not give fire with ſteel.

Fracture—Lateral fracture imperfectly conchoidal—Tranſverſe fracture ſlaty, paſſing into the coarſe-grained earthy.

Gave a white ſtreak.

Effervesced with acids.

Gave an earthy ſmell when breathed on.

When calcined did not flake.

Colour, when calcined, a yellowiſh grey.

Cracked, and flew into thin ſlates, when expoſed to a low heat in an open fire.

Specific gravity, at the temperature of 66 Fahrenheit 2. 68.

CHEMICAL

CHEMICAL ANALYSIS.

One hundred parts contain the following ſubſtances in nearly the proportions annexed:

Carbonate of lime - -	68
Oxyd of iron - - -	2
Argill - - - -	$7\frac{1}{2}$
Silex - - - -	18
Carbon and Bitumen - -	3
Water - - - -	$1\frac{1}{2}$

The proceſs by which a mineral ſubſtance, of the nature of that which is the ſubject of this paper, is analyſed, is ſo ſimple, and well known, as almoſt to render any detail of the operation unneceſſary. More than one analyſis was made. The muriatic and acetous acids were ſeverally uſed to take up the carbonate of lime; but the former diſſolved the iron alſo. The metal was precipitated from the muriatic ſolution by pruſſiate of pot-aſh, and the lime ſeparated from each by carbonate of pot-aſh. The argill was extricated with difficulty, by long digeſtion and boiling in ſulphuric acid, and precipitated by carbonate of ſoda.—Beſides the general analyſis, each ingredient was ſeparately collected from diſtinct portions, of 100 grains each, of the ſpecimen.

The

The following are my reaſons for ſuppoſing that the iron was in the ſtate of an oxyd, and that the principal colouring matter was compoſed of carbon and bitumen.

With a view of diſcovering whether the iron was in its metallic ſtate, or otherwiſe, in the ſtone, a large quantity of it, amounting to ſeveral hundred grains, was treated with muriatic acid, and the gas which was extricated was paſſed through water, and caught in a pneumatic apparatus.

The ſame proceſs was repeated, with the ſubſtitution of the ſulphuric for the muriatic acid. In both caſes the reſult was the ſame.—No hydrogen gas could be collected; but, when the whole of the carbonic acid had been imbibed by the water, there remained, in the veſſel, a ſmall quantity of azotic gas.

That the dark colour of the ſtone could not be produced merely by the oxyd of iron, I was naturally led to ſuſpect from the change produced in it by calcination. I therefore ſatisfied myſelf on that point, by carefully ſeparating from a portion of the ſtone every particle of the metal, a proceſs which produced no change in its colour.

As it appeared now pretty evident that the calp contained ſome inflammable matter, to which it was indebted for its dark colour, I endeavoured to eſtimate its proportion by the following proceſs:

Thirty

THIRTY grains, from which all the carbonate of lime and oxyd of iron had been previouſly extracted, were dried in a cloſe veſſel, and in ſuch a heat as ſeemed ſufficient to expel the water, and afterwards calcined. By the calcination the powder was rendered white, and loſt that proportion of its weight which I aſſign in the reſult of the analyſis to the carbon and bitumen.

STILL, however, it remained to determine in what ſtate, whether of carbon or of bitumen ſingly, or whether of a mixture of both, the inflammable colouring matter exiſted in the mineral.

ON the ſuppoſition that it was merely carbon in a ſtate of extreme diviſion, it was boiled (the lime, argill and iron having been previouſly ſeparated) in concentrated nitrous acid. By this treatment the intenſity of the colour was ſomewhat diminiſhed, and a ſmall quantity of nitrous air was produced.

THE next attempt was to ſeparate the bitumen, ſuppoſing that it contained any.—To effect this, a large quantity of the powdered calp was boiled in cauſtic pot-aſh, filtered and edulcorated; but as the reſiduum on the filtre retained the original colour, it conſequently could not have been entirely produced by bituminous matter.

THAT it contained, however, *ſome* bitumen, ſeems pretty evident; for, having evaporated the filtered ſolution to dryneſs, and diſſolved

diſſolved the dried reſiduum in muriatic acid, an oily matter floated on the ſurface of the liquor.

From theſe experiments I have ventured to aſſert, that the colouring matter is compoſed, in proportions which I have not been able to aſcertain, of a mixture of bitumen and carbon.

With reſpect to the azot I am unable to form any ſatisfactory conjectures of its origin. Might it not have proceeded from the body of the animal whoſe ſhell had contributed the carbonate of lime to the formation of the maſs?

The following facts may aſſiſt enquiries upon this ſubject:

Calp is found in great quantities in the neighbourhood of Lucar; and the quarries from which it is dug generally exhibit the following appearance:

Immediately under the vegetable mould is a thin bed of lime-ſtone gravel; beneath which, to a conſiderable depth, are ſtrata of dark lime-ſtone, ſeparated from each other by beds or layers of argillaceous ſhiſtus. The deeper the quarry is dug, the nearer the lime-ſtone ſeems to approach to the nature of calp; to which it at length arrives, by a gradual and ſcarcely perceptible tranſition.

In the grounds of Edmondſbury, ſcarcely one mile from Lucan, there is a ſpring, called by the common people the Boiling Well, which

which is of a ſomewhat higher temperature than the neighbouring ſprings, and from the bottom of which are continually riſing large bubbles of pure azotic gas.

In an analyſis, which I ſometime ſince attempted, of the Lucan water, I found that azotic as well as hydrogen gas eſcaped from it at a boiling heat; a circumſtance which could hardly have been occaſioned by the decompoſition of atmoſpheric air held in ſolution in the water, as no ſulphuric or ſulphureous acid was produced, as was to be expected from the ſimultaneous decompoſition of ſulphurated hydrogen gas and atmoſpheric air.

It may alſo be worth obſerving by perſons who are deſirous of proſecuting this enquiry farther, that in moſt of the lime-ſtone in that neighbourhood, which contains ſhells, is to be found ſulphurated hydrogen.

And, likewiſe, that after evaporating the Lucan water to dryneſs, a ſmall quantity of a fetid bituminous matter, ſoluble in alcohol, remains mixed with the earths and ſalts.

On theſe few facts I ſhall not preſume to form a theory—Neither do I offer them as ſubjects for ſpeculation, but as an encouragement to further and more accurate reſearches.

Note.—As I believe that my analyſis of the Lucan water is a nearer approximation to the truth than any hitherto publiſhed, and perhaps ſufficiently accurate for medical purpoſes, I ſhall here ſubjoin it.—To thoſe who are acquainted with the difficulties attending the inveſtigation of the ingredients of an hepatic water, it will not afford any ſurpriſe that I ſhould offer the reſult of mine with diffidence.

LUCAN WATER, TWO GALLONS.

	GRAINS.
Carbonate of magneſia -	1½
Carbonate of lime - -	23
Carbonate of ſoda - -	39
Muriate of ſoda - -	4
Sulphur - - - -	16
Bitumen - - -	

The carbonate of lime is held in ſolution by an exceſs of carbonic acid, amounting to about 32 cubic inches in two gallons of the water.—The ſulphur is in the ſtate of ſulphurated hydrogen.

On the ORBITS *in which* BODIES *revolve, being acted upon by a* CENTRIPETAL FORCE *varying as any* FUNCTION *of the* DISTANCE, *when those* ORBITS *have* TWO APSIDES.

By the Rev. J. BRINKLEY, *A. M. ANDREWS Professor of Astronomy in the University of Dublin.*

Read March 9, 1801.

THE investigation of orbits described by bodies acted upon by any centripetal force whatever is reduced by Sir Isaac Newton to the quadrature of curves (8 sect. lib. 1. Princip.) The quadrature of such curves as arise from the application of his method can only in few instances be completely accomplished. A portion of the area of any curve may be easily found by a converging series, but not so the whole area. To approximate to the whole area is in most cases very difficult; and hitherto the orbits have been investigated for very few laws of force. By the method here proposed it is shewn, that when the orbit has two apsides, that whatever be the function of the distance which expresses the law of the centripetal force, the orbit may be determined by a series of sines of multiple arcs converging by the powers of the excentricity. From hence the angle between the

apſides is immediately determined, which is one of the moſt intereſting reſults of this method. For we have not only all that is determined in the laſt propoſition of the 9th ſection of the Principia, but alſo the motion of the apſides for excentric orbits. The method in the 9th ſection gives only the limit of the motion of the apſides. It cannot be applied to find the motion in excentric orbits; which muſt in ſome meaſure be conſidered as a defect. The limit of the motion of the apſid is never required, for then the orbit is a circle; but the motion before it has arrived at its limit. The motion indeed approximates indefinitely to the limit, but this is not ſo evident from the method of Newton; we know from that *only* the limit, and nothing of its antecedent ſtate. It muſt not be underſtood, that it is here intended to object to the truth of the reaſoning in the 9th ſection; the ingenuity there ſhewn by the illuſtrious author is truly admirable, and is perhaps in no part of the Principia more ſtriking. His penetrating mind, doubtleſs, ſaw at once the full force of that reaſoning. It has, however, been a ſubject of difficulty to ſome. Walmſly, a very acute mathematician, found from the ſame data as in the 2 Cor. 45 Prop. a double motion of the apſides, and therefore conſonant to the motion of the lunar apogee. He even has been followed by the ingenious Friſius, who, correcting, as he imagined, ſome defects in Walmſly's ſolution, found the ſame reſult as Walmſly. From which it would follow, that the mean motion of the lunar apogee could be found from the conſideration

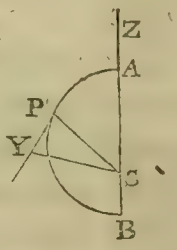
ration of a centripetal force, varying in a compounded ratio of the diſtance, and conſequently that the 2d Cor. of the 45th Propoſition of the 1ſt Book of the Principia is erroneous. The common error in the ſolutions of Walmſly and Friſius is hereafter pointed out.

The examples of the method here uſed are given in the 2d and 3d propoſitions. The 1ſt propoſition proves, that an orbit, having two apſides, and deſcribed by a body impelled by a force varying inverſely as the ſquare of the diſtance, is an ellipſe. This ſolution is probably more direct than any other of the ſame propoſition, and has a connection with what follows. The ſeries in the 2d and 3d propoſitions are not continued beyond the 2d and 3d powers of the excentricity. This was ſufficient for the motion of the apſides, and there can be no difficulty in continuing the terms.

It ſeems hardly neceſſary to obſerve, that when the force varies as any function of the diſtance, an orbit can have only two different diſtances of the apſides from the centre, becauſe it muſt be ſimilar on each ſide of the apſid. The law of the force alſo readily ſhews, whether any orbit deſcribed by that law can have two apſides, by comparing it with the law of the centrifugal force, which always varies in the inverſe triplicate ratio of the diſtance. Therefore if the force varies in any direct ratio, or any inverſe ratio leſs than the triplicate, the orbits deſcribed may have two apſides.

Prop.

PROP. 1. The centripetal force varying inverſely as the ſquare of the diſtance to determine the orbit when it has two apſids.



SOLUTION. Let the greateſt diſtance S A $= 1 + e$ and the leaſt diſtance S B $= 1 - e$. Any diſtance S P $= x$, the perp. S Y let fall on the tang. $= p$, the angle A S P $= a$, and the diſtance S Z from which a body acted upon by the centripetal force muſt fall to acquire the velocity at A $= z$.

THEN (Cor. 1. Prop. 1. Sect. 1. and Cor. 2. 40 Prop. 8 Sect. 1 Lib. Newtoni Prin.)

$\overline{1+e}^2 : \overline{1-e}^2 :: \frac{1}{1-e} - \frac{1}{x} : \frac{1}{1+e} - \frac{1}{x}$ whence $z = 2$

and $\because p^2 : \overline{1+e}^2 :: \frac{1}{1+e} - \frac{1}{2} : \frac{1}{x} - \frac{1}{2}$ or $p^2 = \frac{x}{2-x} \times \overline{1-e}^2$

Now $\dot{a} = \text{A S } \dot{\text{P}} = \frac{\text{S P} \times \text{S Y}}{\text{P Y} \times \text{S P}} = \frac{-\dot{x}}{x\sqrt{\frac{x^2}{p^2} - 1}}$

$\therefore \dot{a} = \frac{-\dot{x}}{x\sqrt{\frac{x \times 2-x}{1-e^2} - 1}} = \frac{-\dot{x}}{x^2\sqrt{\frac{2}{x} - 1}\sqrt{\frac{x}{1-e^2} - \frac{1}{x^2}}}$. For finding the

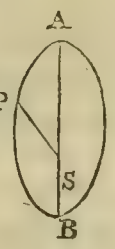
fluent of this fluxion let $v = \frac{1}{1-e^2} - \frac{1}{x}$ and $\dot{a} = \frac{-\dot{v}}{\sqrt{e^2 - v^2}}$

whence

Whence a = the arc, the cofine of which is $\frac{v}{e} \times \overline{1-e^2}$ and rad. unity = the arc, the cofine of which is $\frac{1}{e} - \frac{1-e^2}{ex}$. This fluent requires no correction, becaufe when $a = o$, $\frac{v}{e} \times \overline{1-e^2} = 1$. And becaufe $cs, a = \frac{1}{e} - \frac{1-e^2}{ex}$, $\therefore x = \frac{1-e^2}{1-e\,cs,\,a}$, which is the well known equation of the ellipfe, in which x is the diftance from the focus and a the true anomaly Q E I.

For the fubfequent propofitions it is neceffary to folve the following problem :

Problem. Let the greateft diftance S A from the focus S of the ellipfe A P B = $1+e$, the leaft diftance S B = $1-e$, any other diftance S P = $1+y$, and the angle A S P = a, it is required to expref the different integral powers of y by a feries of cofines of multiple arcs and powers of the excentricity.



Solution. The equation of the ellipfe gives

$$1+y = \frac{1-e^2}{1-e\,cs,\,a} \quad \therefore y = e \times \frac{cs,\,a-e}{1-e\,cs,\,a}$$

or

now $\frac{1}{1-e\,cs,\,a} = 1 + e\,cs,\,a + e^2\,cs^2,\,a + e^3\,cs^3,\,a +$ &c.

$\therefore y = -e^2 + \overline{e-e^3}\,cs,\,a + e^2\,cs^2,\,a + e^3\,cs^3,\,a +$ &c. =

$-\frac{e^2}{2} + \frac{e-e^3}{4}\,cs,\,a + \frac{1}{2}e^2\,cs,\,2a + \frac{1}{4}e^3\,cs,\,3a +$ &c. This series might be continued ad libitum from the relation of the terms to each other, which relation may be found by a method well known, but the above is sufficient for the present purpose. Hence

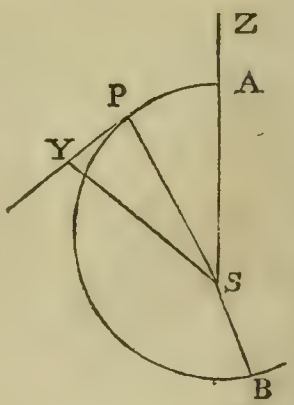
$y^2 = \frac{e^2}{2} - \frac{e^3}{2}\,cs\,a + \frac{e^2}{2}\,cs\,2a + \frac{e^3}{2}\,cs\,3a$ &c.

$y^3 = \frac{3}{8}e^3\,cs,\,a + \frac{1}{4}e^3\,cs\,3a$ &c.

&c. &c.

Prop. 2. The centripetal force varying partly in the inverse duplicate ratio, and partly in the direct simple ratio of the distance to determine the orbit described, when that orbit has two apsides. (Newtoni Prin. Sect. 9. Prop. 45. Cor. 2.)

Solution. Let, as before, the greatest distance $SA = 1 + e$, the least dist. $SB = 1 - e$, the angle $ASP = A$, the distance $SP = x$, the perp. $SY = p$, and let the force at P be expressed by $\frac{1}{x^2} - cx$. Let also SZ be the height from which a body must fall to acquire the velocity at A.



Then

THEN (39 and 40 Prop. 1. Lib. Prin.) $\frac{1}{x} - \frac{1}{z} - \frac{1}{2} c \times \overline{z^2 - x^2}$ may expreſs the ſquare of the velocity at any point P ∴ (1 Cor. 1 Prop. 1 Lib.)

$$\frac{1}{1+e} - \frac{1}{z} - \tfrac{1}{2} c \times \overline{z^2 - \overline{1+e}^2} : \frac{1}{1-e} - \frac{1}{z} - \frac{1}{2} c \times \overline{z^2 - \overline{1-e}^2} :: \overline{1-e}^2 : \overline{1+e}^2$$

Whence $\frac{1}{z} - \frac{1}{2} c z^2 = \frac{1}{2} - \frac{1}{2} c \times \overline{2+2e^2}$

Conſequently $\frac{1}{x} - \frac{1}{2} - \frac{1}{2} c \times \overline{2+2e^2-x^2} : \frac{1}{1+e} - \frac{1}{2} -$

$\frac{1}{2} c \times \overline{2+2e^2 - \overline{1+e}^2} : \overline{1+e}^2 : p^2$

or $\frac{x^2}{p^2} = \frac{2x - x^2 - c x^2 \times 2 + 2e^2 - x^2}{1 - e^2 - c \times 1 - e^2}$

And ∴ $\dot{A} = \frac{-\dot{x}}{x^2 \sqrt{\frac{x^2}{p^2} - 1}} = \frac{-\dot{x}}{x^2 \sqrt{\frac{2x - x^2 - c x^2 \times 2 + 2e^2 - x^2}{1 - e^2 - c \times 1 - e^2} - 1}}$

THE fluent of this quantity may be approximated by help of an ellipſe, the greateſt and leaſt focal diſtances of which are $1+e$ and $1-e$, and a the angle included between the greateſt

distance $1+c$ and any distance x. In this ellipse (see the preceding prop.) $\dot{a} = \frac{-\dot{x}}{x\sqrt{\frac{2x-x^2}{1-e^2}} - 1}$

$\therefore \dot{A} : \dot{a} :: \sqrt{\frac{1-c \times 1-e^2}{1-c \times \frac{2+2c^2x^2-x^4-1-e^2{}^2}{2x-x^2-1-e^2}}}$ (P) (Q) : 1. Now because two roots of the equation $P = o$, viz. $1+e$ and $1-e$ are the roots of the equation $Q = o$, it follows that Q must be a divisor of P; accordingly we find $\frac{P}{Q} = \overline{x+1}^2 - e^2 = \overline{2+y}^2 - e^2$, putting $1+y=x$

Hence $\dot{A} = \dot{a}\sqrt{\frac{1-c}{1-4c+ce^2}} \times \frac{1}{\sqrt{1-c \cdot \frac{2y+y^2}{1-4c+ce^2}}}$

$= \dot{a}\sqrt{\frac{1-c}{1-4c+ce^2}}$ (B) $\times 1 + \frac{c}{2B} \cdot \overline{2y+y^2} + \frac{3c^2y^2}{2B^2} +$ &c. regarding only the 2d power of the eccentricity, which is sufficient for the purpose for which the proposition was designed. Next substituting for y, y^2, &c. the values found in the preceding problem, and taking the fluent, we have

$$A = a\sqrt{\frac{1-c}{1-4c+ce^2}} \times 1 - \frac{\frac{ce^2}{4} - ce\,s,a - \frac{3}{8}\,ce^2\,s,\,2a}{1-4c+ce^2} + \frac{\frac{3}{4}e^2c^2 + \frac{3}{4}e^2c^2\,s,\,2a}{1-4c+ce^2}, \text{ &c.}$$

where

where a is an arc the cosine of which is $\frac{1}{e} - \frac{1-e^2}{ex}$ rad. being $= 1$. Hence for every dist. S P we have the angle A S P, and consequently the orbit is determined. When the body comes to the lower apsid, $a = 180^\circ$ $\therefore$ the angle between the apsides $= 180^\circ \sqrt{\frac{1-c}{1-4c+ce^2}} \times 1 - \frac{ce^2}{4.1-4c+c_2 c} + \frac{3e^2c^2}{4.1-4c+e^2c^2}$, &c.

The limit of this quantity is $180^\circ \sqrt{\frac{1-c}{1-4c}}$ (2 Cor. 45 Prop.)

This proposition is applicable to the lunar orbit. The limit of the result is the same as found by Sir Isaac Newton. Some authors have conceived Newton's conclusion erroneous, and with the same law of force have found the motion of the apsides twice as great. Walmsly, particularly, has imagined, that the principles of the 9th section give the true angle between the apsides only when the force varies according to a simple law of the distance. In his tract, "De "Inæqual. Lunæ," he finds the motion of the apsides, by computing the time a body takes in acceding towards the centre a space equal to twice the excentricity, when impelled by a force which is the difference of the centrifugal and centripetal forces. This time he compares with half the periodic time, and thence

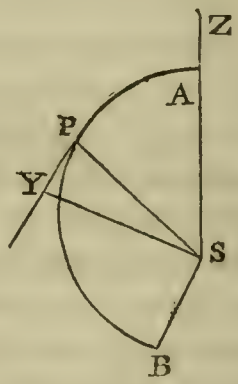
deduces the motion of the apſides to be twice as great as by the principles of the 9th ſection. Friſius, obſerving that Walmſly in his method had omitted the diſturbing tangential force as of no effect in its mean quantity, endeavours to correct his ſolution by uſing the mean velocity of the moon in octants, and her mean periodic time as affected by the tangential force. He then finds the reſult the ſame as Walmſly. But upon examining his method, it will be ſeen it does not differ eſſentially from Walmſly's. Increaſing the velocity, and decreaſing the periodic time, does not affect the angle between the apſides. The motions of the apſides in orbits little excentric, as the above and next propoſition ſhew, almoſt entirely depend upon the variation of centripetal force. The variation of the force in Walmſly's and Friſius's methods will, upon examination, be found to be preciſely the ſame as Newton's, and therefore the motion of the apſides ought to be the ſame. The errors in the proceſſes of Walmſly and Friſius are exactly alike. The ſpace uſed by them for finding the time is only an approximation to the excentricity. At the end of the ſpace the velocity is evaneſcent, and from that circumſtance the fluent of the fluxional expreſſion of the time muſt be erroneous.

Taking e=,055, as in the lunar orbit, the angle between the apſides will differ about 4 ſeconds from the limit, conſequently the

the error in the mean motion of the apſides of the lunar orbit, by neglecting the excentricity, is only 8 ſeconds in a revolution.

In the lunar orbit referred to the ecliptic, the perturbing force in the direction of the radius vector is expreſſed by a function of that radius vector, and of the angular diſtance of the moon from the ſun; and the perturbing force in a direction perp. to the radius vector, is expreſſed by another function of the ſame quantities. The former force in its mean quantity is expreſſed by a function of the radius vector only. The mean quantity of the latter = o. It has therefore been often imagined, that the mean motion of the lunar apogee might be inveſtigated, by conſidering the moon acted upon by a centripetal force, expreſſed by a function of the diſtance only. The arguments for this opinion are certainly plauſible, but have by no means the weight of demonſtration. The reſult ſhews, that ſuch an opinion reſts upon no ſolid foundation. It does not appear to be poſſible to inveſtigate the mean motion of the lunar apſides, except from the general expreſſions of the forces in direction of the radius vector and in the direction perp. thereto.

PROP. 3. The centripetal varying as the $n-1$ power of the diſtance to determine the orbit deſcribed when it has two apſides.



SOLUTION. Let S A $= 1+e$ and S B $= 1-e$. Any diſtance S P $= x$, S Y $= p$ and the $\angle$ A S P $=$ A. Let alſo Z A be the ſpace thro' which a body muſt fall to acquire the velocity at A, and let Z S $= z$

Then (40 Prop. Cor. 2. and 1 Prop. Cor. 1. Prin. Newtoni)

$z^n - \overline{1+e}^n : z^n - \overline{1-e}^n :: \overline{1-e}^2 : \overline{1+e}^2$

or compon. $z^n - \overline{1+e}^n : \overline{1+e}^n - \overline{1-e}^n :: \overline{1-e}^2 : \overline{1+e}^2 - \overline{1-e}^2$

Whence $z^n = \frac{\overline{1+e}^{n+2} - \overline{1-e}^{n+2}}{4e} = \frac{n+2}{2} + \frac{\overline{n+2}.\overline{n+1}.n}{2.2.3} e^2 +$

$\frac{n+2.n+1.n.n-1.n-2}{2.2.3.4.5.} e^2$ &c.

and becauſe $z^n - x^n : z^n - \overline{1+e}^n :: \overline{1+e}^2 : p^2$

we

we have $\dot{A} = \frac{-\dot{x}}{x\sqrt{\frac{x^2}{p^2}-1}} = \frac{-\dot{x}}{x\sqrt{\frac{z^n x^2 - x^{n+2}}{z^n - 1+e^n \times 1+e^2}-1}}$

For finding the fluent of this fluxion we may uſe the ellipſe as in the laſt prop.

and $\dot{A} : \dot{a} :: \frac{1}{\sqrt{\frac{z^n x_2 - x^{n+2}}{z^n - 1+e^n \times 1+e^2}-1}} : \frac{1}{\sqrt{\frac{2x-x^2}{1-e^2}-1}}$

or $\dot{A} = \dot{a}\sqrt{\frac{z^n - 1+e^n \times 1+e^2}{1-e^2}}\,(R) \times \frac{1}{\sqrt{\frac{x^{n+2} - z^n x^2 + z^n - 1+e^2 \times 1+e^2}{x^2 - 2x + 1 - e^2}\,(P)}\,(Q)}$

Now $1+e$ and $1-e$ are values of x in the equation $P = o$, and the ſame quantities are the values of x in the equation $Q = o$ ∴ Q muſt be a diviſor of P whatever be the value of n. To accompliſh this diviſion let $x = 1+y$

and then $P = \left.\begin{matrix} -z^n \times \overline{1+y}^2 + \overline{1+y}^{n+2} \\ +z^n \times \overline{1+e}^2 - \overline{1+e}^{n+2} \end{matrix}\right\} =$

$$-z^n \times \overline{2y-2e} - z^n \times \overline{y^2-e^2} + \overline{n+2.y-e} + \frac{\overline{n+2.n+1}}{1.2}\,\overline{y^2-e^2} +$$

$$\frac{\overline{n+2.n+1.n}}{1.2.3}\,y3 - e3 + \text{&c.}$$

=

$$= -z^n \times \overline{y^2 - e^2} + \frac{\overline{n+2}\cdot\overline{n+1}}{1.2}\,\overline{y^2 - e^2} + \frac{\overline{n+2}.\overline{n+1}.n}{2.3}\,\overline{y^3 - e^2 y} +$$

$$\frac{\overline{n+2}.\overline{n+1}.n.\overline{n-1}}{2.3.4}\,\overline{y^4 - e^4} + \frac{\overline{n+2} \cdots \overline{n-2}}{2.3.4.5}\,\overline{y^5 - e^4 y} + \text{\&c.}$$

each term of this quantity is divisible by $y^2 - e^2 = Q$, because $y^{2m} - e^{2m}$ (m being a whole number) is always divisible by $y^2 - e^2$

$$\therefore \frac{P}{Q} = -z^n + \frac{\overline{n+2}.\overline{n+1}}{1.2} + \frac{\overline{n+2}.\overline{n+1}.n}{2.3}\,y + \frac{\overline{n+2}\,\overline{n+1}.n.\overline{n-1}}{2.3.4}\,\overline{y^2 + e^2} +$$

$$+ \frac{\overline{n+2} \cdots \overline{n-2}}{2.3.4.5}\,y^3 + e^2 y + \frac{\overline{n+2} \cdots \overline{n-3}}{2.3.4.5.6}\,y^4 + e^2 y^2 + e^4 + \text{\&c.} =$$

$$\frac{\overline{n+2}.n}{1.2} \times : 1 + \frac{\overline{n+1}.\overline{n-3}}{3.4}\,e^2 + \frac{\overline{n+1}.\overline{n-1}.\overline{n-2}.\overline{n-6}}{3.4.5.6.}\,e^4 + \frac{n+1}{3}\,y +$$

$$\frac{\overline{n+1}.\overline{n-1}.}{3.4}\,y^2 + \frac{\overline{n+1}\,\overline{n-1}.\overline{n+2}}{3.4.5}\,y^3 + e_2 y + \frac{\overline{n+1}\,\overline{n-1}\,\overline{n-2}.\overline{n-3}}{3.4..5.6}$$

$$\overline{y^4 + e^2 y^2} \quad \text{\&c.}$$

Alſo $\frac{1}{R} = \frac{2}{n} \times 1 - \frac{\overline{n+1}.\overline{n-4}}{2.3}\,e^2$ &c. It is evident from the proceſs for the values of R and $\frac{P}{Q}$ that only the even powers of the eccentricity can enter into thoſe values.

Subſtituting

Subſtituting theſe values, $\dot{A} = \dot{a}\sqrt{\frac{RQ}{P}} =$

$$\frac{\dot{a}}{\sqrt{n+n}} \times 1 - \frac{n+1.n-5}{3.4}e^2 + \frac{n+1}{3}y + \frac{n+1.n-1}{3.4}y + \frac{n+1.n-1.n-2}{3.4.5}y^3 + e^2y - \frac{n+1^2.n-4}{2.3.3}e^2y \text{ &c.}\Big)^{-\frac{1}{2}}$$

$$= \frac{\dot{a}}{\sqrt{n+2}} \times : 1 + \frac{n+1.n-5}{2.3.4}e^2 - \frac{n+1}{2.3}y + \frac{n+1}{3.4}y^2 + \left.\begin{array}{l} \frac{n+1^2.n-1}{3.4.4} \\ -\frac{n+1.n-1.n-2}{2.3.4.5} \\ -\frac{5.n+1^3}{3.4.4.9} \end{array}\right\} y^2 \quad \left.\begin{array}{l} +\frac{n+1^2.n-4}{2.2.3.3} \\ -\frac{n+1^2.n-5}{3.4.4} \end{array}\right\} e^2y \text{ &c.}$$

Next ſubſtituting for y, y_2, y_3, &c. their values found in the foregoing problem, and taking the fluent we have

$$A = \frac{a}{\sqrt{n+2}} \times : 1 + \frac{n+1}{2.3} \times : \frac{n-2}{4}e^2 - es, a + \left.\begin{array}{l} \frac{17.n+1.n-1}{8.8} \\ -\frac{5.n+1^2}{3.4.4} \\ -\frac{11.n-1.n-2}{4.5.8} \end{array}\right\} e^3, sa \quad -\frac{e^2}{2.4}s, 2a +$$

$$\left.\begin{array}{l} \frac{n+1.n-1}{2.3.3.4.4} \\ -\frac{n-1.n-2}{3.4.4.5} \end{array}\right\} e^3 s, 3a, \text{ &c.}$$ where a is an arc, the coſine of which is $\frac{1}{e} - \frac{1-e^2}{ex}$ and rad. 1. This is the general equation for any diſtance and angle. When the body arrives at the lower

apſid, $a = 180^{\circ}$ and $s, a; s, 2a$ &c. $= o$ ∴ the angle between the apſides =

$$\frac{180^{\circ}}{\sqrt{n+2}} \times 1 + \frac{\overline{n+1}\cdot\overline{n-2}}{2\cdot 3\cdot 4} e^2, \text{ \&c.}$$

in which only the even powers of the eccentricity can enter. It is evident from this expreſſion that the motion of the apſides will be always affected by the eccentricity of the orbit, unleſs either $n+1=o$ or $n-2=o$ or that the force varies either in the inverſe duplicate ratio of the diſtance or in the direct ſimple ratio. We know from other principles that in theſe two laws the eccentricity does not affect the angle between the apſides, but it is a very remarkable circumſtance that this takes place for no other law.

The above propoſitions point out how the orbit may be found whatever be the function of the diſtance expreſſing the centripetal force. For z will be always found a function of e, and therefore p will always be a function of x and e, conſequently $\frac{x^2}{p^2} - 1$ will be a function of x and e, or ſubſtituting for x, $1+y$, $\frac{x^2}{p^2} - 1$ will be a function of y and e. And two roots of the equation

equation $\frac{\overline{1+y}^2}{p^2} - 1 = o$ are $\pm e \therefore y^2 - e^2$ will be always a divisor of $\frac{x^2}{p^2} - 1$ or $\frac{\overline{1+y}^2}{p^2} - 1$. Whence in every case we may proceed as above and shall always have the equation of the orbit in a series of sines of multiple arcs converging by the powers of the excentricity.

OBSERVATIONS *and* EXPERIMENTS *undertaken with a view to determine the* QUANTITY *of* SULPHUR *contained in* SULPHURIC ACID; *and of this* LATTER *contained in* SULPHATES *in general. By* RICHARD CHENEVIX, *F. R. S. and M. R. I. A.*

Read, May 4, 1801.

IN a paper which I had the honour to preſent to the Royal Society of London, and the ſubject of which was the analyſis of ſome arſeniates of copper, and of iron, I had occaſion, in examining many pyrites, matrices of thoſe ores, to remark the very great inequality which prevailed in the reſults of repeated experiments, made with a view to determine the proportion of ſulphur. But I ſoon perceived, that the inaccuracy was cauſed by a partial combuſtion and acidification of the radical, through the means of the nitric acid, employed to diſſolve the ore.

HAVING therefore, in the uſual manner, aſcertained what quantity of that ingredient remained untouched, I was forced to

to ſeek the reſt of it in the liquor, which had waſhed the various precipitates. To obtain it, I poured a ſolution of nitrate of Barytes into thoſe waſhings, when all the other ſubſtances had been carefully ſeparated, and was thereby enabled to precipitate, in a ſtate of purity, the ſulphate of Barytes, formed by that earth, and by the portion of ſulphur originally acidified, in the firſt treatment of the ore by nitric acid. To come at the knowledge of the proportion of ſulphur, contained in a given quantity of ſulphate of Barytes, I had recourſe, in the firſt inſtance, to the quantity of ſulphur, ſaid by Lavoiſier to be contained in ſulphuric acid, and, in the next, to the proportions of the latter, announced in the ſynoptic tables of Fourcroy, as entering into the compoſition of ſulphate of Barytes. According to the former of thoſe chymiſts, 100 parts of ſulphuric acid contain 71 of ſulphur and 29 of oxygen; and again, in adopting the proportions of the latter, we have 33 per cent. of acid, on ſulphate of Barytes. But if 100 contain 71 of ſulphur 33 muſt contain 23.43. Conſequently, for every 100 parts of ſulphate of Barytes, I was to allow 23.43 of ſulphur. But, by the reſults of my analyſes, I had ſuch quantities of ſulphate of Barytes, as induced me to doubt the accuracy of one or other of the ſtatements, by which I eſtimated the quantity of real ſulphur contained in the ore.

No

No perſon is better acquainted than our celebrated Preſident, with the many difficulties that occur in the analyſis of ſalts in general; particularly with regard to the quantity of real acid they may contain. It has been a work of trouble to the ableſt chymiſts, and they have not always agreed in their reſults. The proportions, announced by Fourcroy, may therefore be doubted, in common with thoſe of the other learned operators, to whom I have alluded.

The real quantity of acid, produced by the combuſtion of any acidifiable baſis, can be determined by one or other of the following methods only: by direct combination, in ſome ſalt, the proportions of which are already known; or by obtaining, in a ſtate perfectly free from water, the acid reſulting from ſuch combuſtion. To the former method, the general objections againſt all analyſes of ſalts muſt apply. The latter is ſtill more defective. It is by no means certain, that we have ever yet obtained any acid, in a ſtate of perfect ſiccity, unleſs we except the phoſphoric and the arſenic; for even the cryſtallized vegetable acids retain a portion of water in their cryſtallization. It is not that I abſolutely deny our having obtained them ſo; but I ſay merely, that we have no proof. It would indeed be ſetting narrow bounds to the perfection of nature, to aſſert, that no combuſtible body could, when ſaturated with oxygen,

oxygen, aſſume, of itſelf, the ſtate of liquidity; or that the oxide of the particular ſubſtance, called hydrogen, muſt be preſent to confer that property. Doubtleſs, ſulphuric acid may, as well as water, contain, in itſelf, ſo juſt a proportion of ſpecific heat, as to remain liquid at the temperature of our globe, and under the preſſure of our atmoſphere. But, both water and ſulphuric acid being eaſily volatilized, and having a powerful affinity for each other, it is not eaſy, if even poſſible, by diſtillation, to ſeparate them with ſufficient accuracy, in experiments of delicate inquiry. A ſecond ſource of error, therefore, remained open in this branch of the calculation, which gave the proportion of 23.43 of ſulphur, as that contained in 100 parts of ſulphate of Barytes.

However, before I would allow myſelf to call in queſtion ſuch authorities as thoſe I have quoted, I inſtituted the following experiments: In a tubulated glaſs retort, I put 100 parts of purified ſulphur, and poured upon them ſtrong nitric acid. A quilled receiver, plunging into a Woulfe's apparatus, was adapted to the retort; and, all being well luted, I proceeded to diſtil. The liquor, which came over, was poured back ſeveral times upon the ſulphur, until the whole was diſſolved. The water, which had come over, and that, through which the nitrous gas, produced during the operation, had paſſed, were eſſayed

eſſayed for ſulphureous acid, and no traces of it could be found. No ſulphur had been volatilized; therefore no ſuſpicion could remain, that all was not converted into ſulphuric acid. The liquors, which were in the various parts of the apparatus, were united; and to them was added a ſufficient quantity of nitrate of Barytes. The whole was evaporated gently; becauſe, though I am well acquainted with the very little ſolubility of ſulphate of Barytes, I well know that nitric acid will retain a ſmall portion of it, particularly when formed in a liquor where that acid abounds. In a firſt experiment, I obtained 694 from 100 of ſulphur, in a ſecond, 348 from 50, and in a third, 347 from the ſame quantity. But the ſimple rule of three reduced theſe quantities to 14.6, or 14.4 per cent. of ſulphur, contained in ſulphate of Barytes; a difference wholly to be neglected. If, therefore we take 14.5 as the average, for the quantity of ſulphur contained in 100 parts of ſulphate of Barytes, we ſhall not be far from the truth. From the accordance of theſe experiments, repeated and varied, I had now no doubt, but concerning the ſource where I was to ſeek the error, which gave 23.43 as the juſt proportion.

To aſcertain this point, I operated in the following manner: I prepared ſome lime, as pure, I believe, as chymical means can procure it. I digeſted white marble in muriatic acid; and,

by leaving an excefs of the earth, was certain, that by the ſuperior affinity of lime for that acid, nothing elſe had been taken up. Upon trying the ſolution with ammonia, no precipitate took place. By means of carbonate of potaſh, I ſeparated the lime in the ſtate of carbonate; and, after well waſhing the precipitate, expoſed it in a platina-crucible to a violent heat, till the weight no longer diminiſhed. I am acquainted with no more efficacious method to prepare lime, fit for the delicate purpoſes of ſcientific chymiſtry.

One hundred parts of this lime were diſſolved in dilute muriatic acid, in the ſame platina-crucible, previouſly weighed; and then ſulphuric acid was added in ſufficient quantity. Sulphate of lime was precipitated; and the veſſel was expoſed to a heat, at firſt gentle, to evaporate the liquor; and then, by degrees, raiſed to a temperature, which could expel every thing but the combined ſulphuric acid, and leave the ſulphate of lime completely calcined. The crucible with the ſalt was then weighed and the augmentation was 76.—It appears to me, that, if we admit (and I ſee no reaſon that we ſhould not admit it) that calcined lime and calcined ſulphate of lime are wholly exempt from water, it muſt be clear, that the 76 additional weight were ſulphuric acid; and, that the ſulphuric acid muſt in this ſtate, more than in any other, approach nearer to what may be termed,

termed, abſolutely real acid. One hundred parts of calcined ſulphate of lime contain therefore

Lime - - -	57
Sulphuric acid - - -	43
	100

By the former experiments (thoſe made upon ſulphur converted into acid, and then united to Barytes) we had the quantity of ſulphur, contained in ſulphate of Barytes. By the latter (thoſe made by directly combining lime with ſulphuric acid) we had the proportion of real acid, contained in calcined ſulphate of lime. Conſequently, by knowing the ratio, that ſulphate of Barytes bears to ſulphate of lime, with regard to the acid in each, we ſhall arrive at the knowledge of the quantity of ſulphur, contained in real ſulphuric acid. For this purpoſe, I attempted to diſſolve, in water, 100 parts of ſulphate of lime. But finding, in this method of proceeding, a conſiderable inconvenience ariſing from the great quantity of liquor, neceſſary to effect the ſolution of that ſalt, I had recourſe to the following expedient. Upon 100 grains of calcined ſulphate of lime, I poured ſome oxalic acid, which attracts the baſis with an affinity ſuperior to that exerciſed by ſulphuric acid. Oxalate of lime was here formed; but oxalate of lime is ſoluble in a very ſmall exceſs of any acid. A little muriatic acid operated a com-

plete ſolution; and thus a great quantity of ſulphate of lime required but little water to diſſolve it. Into this liquor, muriate of Barytes was poured, and ſuffered to remain ſome time, gently heated. By theſe means, any oxalate of Barytes, that might have been formed, was retained in ſolution, by the original exceſs of acid; and the entire quantity of ſulphate of Barytes was depoſited. Of the exactneſs of all theſe methods, which I uſed, as the inſtruments by which I aſcertained theſe reſults, I convinced myſelf by various preliminary experiments. After the uſual filtration, waſhing and drying at the gentle heat of a ſand-bath, I obtained in one experiment 185, in another 183, and, laſtly, in another 180. This difference does not exceed the limits of what all perſons, converſant in analytic chymiſtry, will allow to experiments of this nature. We may therefore take 183 as the mean proportion; conſequently, we ſhall ſay, that 183 of ſulphate of Barytes contain the ſame quantity of ſulphuric acid, as 100 of ſulphate of lime; and 183 : 43 :: 100 : 23.5. Therefore 23.5 is the proportion of acid in 100 of ſulphate of Barytes. But we have before ſeen, that 14.5 of ſulphur, acidified by nitric acid, form that portion of ſulphuric acid contained in 100 of ſulphate of Barytes: viz. 23.5. We muſt now ſay, that 23.5 : 14.5 :: 100 : 61.5, and the fourth term will be the proportion of ſulphur = 61.5, which combined with 38.5 of oxygen will form 100 of real ſulphuric acid.

In neither of the proportions, whether it be of the acid contained in the ſalt, or of the combuſtible baſis contained in the acid, do I agree with the two chymiſts whom I have quoted. This juſtly excited ſome doubts in my mind, and led me to repeat my experiments. Nor ſhould I yet be thoroughly ſatisfied, if I could not, upon other grounds, than by ſuppoſing inaccuracy in them, account for the apparent differences. We muſt ever expect to ſee the errors of our predeceſſors corrected by men, much inferior in abilities; but who, by poſſeſſing more certain means, ſupply the want of genius and invention. At the time in which the experiments were made, that determined the proportion of 33 per cent. of ſulphuric acid in ſulphate of Barytes, it was not known that we had never obtained any Barytes pure; and that a conſiderable portion of carbonic acid reſiſted the action of every degree of heat that had been applied to carbonate of Barytes. The fact was, I believe, firſt obſerved by Pelletier; but the method of avoiding the inconvenience was pointed out by Vauquelin. He decompoſes nitrate of Barytes by lime, and a moderate degree of heat is ſufficient to expel all the acid and the water. The chymiſts, I have mentioned, performed ſynthetic experiments, by combining, directly or indirectly, ſulphuric acid, and ſuch Barytes as they imagined to be pure. The conſtant ſimilarity of their reſults is ſufficient to prove the accuracy of their operations; but working upon an impure ſubſtance, they muſt have been contented with a ſimilarity of error.

THREE cauſes may exiſt which are capable of accounting for any variation, whether in *plus* or in *minus*, that might have appeared in the experiments, by which Lavoiſier determined the quantity of ſulphuric acid, obtained by the combuſtion of ſulphur in oxygen gas.

1ſt. A part of the ſulphur may be volatilized during combuſtion.

2d. ALL the Sulphur may not be converted into ſulphuric acid; but part may remain in the ſtate of ſulphureous acid.

3d. In rectifying, ſome acid may come over along with the water; or, *vice verſâ*, ſome water remain with the acid. Theſe conſiderations will excuſe me for having propoſed a doubt, where the authority of ſo great a man exiſts againſt the experiments which I have related.

THE method which I had uſed to aſcertain the quantity of ſulphur in an ore, had been practiſed by Taſſaert, (Annales de Chimie, No. 82: Analyſis of Cobelt from Tunaberg) but he calculated according to the proportions of Lavoiſier and of Fourcroy. In another paper by Thenard, (Annales de Chimie, No.

No. 96.) he ſtates the proportions of ſulphuric acid, obtained by treating ſulphur with nitric acid, to be

Sulphur - -	55.56
Oxygen - -	44.44
	100.00

but in the extract given by Guyton in that No. of the Annales de Chimie, the mode of operation is not deſcribed. Calcined ſulphate of Barytes is eſtimated in the ſame paper to contain

Barytes - -	74.82
Sulphuric acid - - -	25.18
	100.00

which proportions are as near to what I had found as can be expected; for ſulphate of Barytes does not contain more than 3 per cent. of water of cryſtallization, and they muſt be deducted from the quintal.

Having determined with accuracy the proportion of acid in any inſoluble ſulphate, it is eaſy to proced to the determination of that contained in any other ſulphate. The docimaſtic art or analytic chymiſtry, in general, cannot however expect to derive ſuch advantages from the knowledge of ſoluble ſalts, as

of

of thoſe, which, from their inſolubility, may be uſed with accuracy in delicate experiments to determine the proportions of the conſtituent parts of bodies. But if, with this mode of operating, we compare the quantities of real acid, ſaid (in thoſe excellent tables with which Mr. Kirwan has enriched the ſcience) to be contained in ſulphuric acid of different ſpecific gravities, they will ſerve as proofs of their mutual validity; and perhaps demonſtrate, that ſulphuric acid, as well as water, and ſome other bodies, may of itſelf enjoy liquidity, at the temperature and preſſure which act upon our globe.

METEOROLOGICAL OBSERVATIONS *made at* LONDONDERRY *in the Year* 1800. *By* WILLIAM PATTERSON, *M. D. and M. R. I. A.*

Read, May 4th, 1801.

Tabular Summary of Inſtruments.

Months.	Barometer.		Thermom.		De Luc's Hygrom.		Rain Gauge.
	Higt.	Lowt.	Higt.	Lowt.	Higt.	Lowt.	Inch. Parts.
January - -	29.51	28.85	44½	29	48⅓	47	3. 809596
February -	30.26	29.35	40	34	48	45	0. 630556
March - -	30.36	29.34	49	37	45⅔	40½	0. 630556
April - -	29.78	29.31	51	47	44½	44	4. 019794
May - -	30.26	30.1	57	51	41½	39	1.8911668
June - -	30.33	29.73	67	53	40⅗	36⅗	1. 812848
July - -	30.49	29.80	81	61	40½	35¼	0. 827595
Auguſt - -	30.40	29.94	77	57	40½	30⅓	0. 945834
September -	—	—	—	—	—	—	3. 15078
October -	30.24	29.50	54	48	50⅓	48⅔	4. 965619
November -	30.36	28.92	46	36	50¾	49½	3. 940975
December -	30.18	29.13	47	28	52⅛	49½	2. 601043
	Mean		Mean		Mean		Total
Yearly -	29.82.+		49°.75		42.°19		29.2263628

Note.—The Obſerver regrets that ſo many vacancies appear in the columns of September, which were cauſed by his abſence from home, on profeſſional duties, ſome miles to the north-weſt of Derry, without the poſſibility of having inſtruments for obſervation; but where, nevertheleſs, general notices were made, whilſt the rain-gauge was meaſured by a perſon accuſtomed to attend it.

Tabular

Tabular View of the Direction of the Winds.

Months.	N.	S.	E.	W.	N. W.	N.E.	S.W.	S. E.
January - -	—	4	2	12	2	2	1	14
February -	—	3	3	5	—	—	3	19
March - -	3	3	3	5	1	—	3	17
April - -	3	2	—	15	3	2	2	1
May - -	1	2	4	6	7	1	3	6
June - -	9	2	—	11	14	—	3	—
July - -	2	2	—	23	1	—	4	—
Auguſt - -	7	5	2	17	—	—	2	3
September -	5	3	3	7	4	6	6	3
October - -	3	—	1	11	17	4	3	—
November -	4	—	—	12	23	1	—	2
December - -	4	1	3	12	7	1	6	10
Total -	41	27	21	136	79	27	36	75

A Tabular Statement of the various Phenomena.

Months.	Fair.	Showery.	Wet.	Hail.	Snow.	Froft.	Auor. Boreal.	Thunder and Lightning
January - -	12	16	3	1	7	9	—	—
February -	18	10	—	2	3	10	—	—
March - -	18	13	—	—	3	7	—	—
April - -	4	21	5	2	—	—	—	1
May - -	9	20	2	2	—	—	—	3
June - -	8	20	2	1	—	—	—	1
July - -	17	14	—	—	—	—	—	1
Auguſt - -	15	16	—	—	—	—	—	—
September -	10	16	4	3	—	—	1	4
October - -	6	22	3	2	—	—	—	—
November -	4	23	3	2	4	5	—	—
December -	15	16	—	2	3	18	1	—
Total -	136	207	22	17	20	49	2	10

General

General Remarks.

In *January* there was much blowing weather, ſometimes from the ſouth, but chiefly from the ſouth-eaſt; often accompanied with ſnow; of which deſcription have been our ſevereſt winter ſtorms for ſome years paſt. In this month alſo occurred ſharp congelation, and moſtly with a low barometer. *February* was remarkably dry; the winds were in general moderate; and, although there was frequent congelation, the cold was ſeldom ſevere. Sometimes, indeed, the temperature of the air was almoſt genially warm, even after ſun-ſet, whilſt the preceding day had been perhaps rather ſharp; which was often the caſe during the eaſt and ſouth-eaſt breezes of the whole winter. In *March*, though there were ſome days and nights of congelation, yet only two or three of them were in any degree ſharp; and one only ſo hard as to prevent harrowing in the morning. This, like the preceding month, was remarkably dry; but there were frequent miſts, and ſome thick fogs, which condenſed into drops like rain. There were frequent calms; the winds were generally moderate, ſometimes in different currents, the upper current being ſouth-weſt, whilſt the lower was ſouth-eaſt; which tides of the atmoſphere were frequently noticed during this and the two preceding months. *April* was a wet month, with much blowing, ſqually weather

weather from the ſouth and weſt points, whilſt the temperature of the air was generally moderate. The day, (22d,) on which the thunder and lightning occurred, a very heavy hail ſhower fell. The temperature of the air in *May* was generally cold, with a good deal of blowing weather and frequent ſhowers; to the frequency of which however the quantity of rain that fell did not bear any proportion. The weather of *June* was in general cold; there were only two or three days that could be called genially warm; and during the night of the tenth a degree of refrigeration took place which ſtiffened the cloth on the bleach-fields. The coolneſs of this month may be imputed partly to the prevalence of north-weſt winds, and partly to the ſky being often covered with clouds, which damped the energy of the ſolar rays. The winds in *July* were generally moderate, and the air dry, commonly hot, ſometimes ſultry. In *Auguſt*, though ſeveral ſhowery days are noted, yet the rain was upon the whole very inconſiderable. From the 15th of July to the 12th of this month the weather with little interruption was very hot; and the temperature of the air, in the reſt of theſe two months, was in its general character rather warm, with winds for the moſt part very moderate in their force. *September* was in general a boiſterous month, having ſeveral ſmall gales, the greateſt from the north-weſt, always accompanied with rain, and ſometimes with hail and thunder and lightning. *October*, like the moſt of September, was

was a bluſtry, wet month, but the ſevereſt weather was at night. *November* commenced with a ſtorm from north-weſt, which direction of wind prevailed with ſome violence, accompanied by heavy ſhowers, during the whole month; but the rain occurred principally in the night, as it had done ſince the middle of September. There was alſo ſome hazy weather; and ſome thick fogs took place, which condenſed into drops reſembling rain. On the 25th, after a ſhower of round ſnow, about eight o'clock at night, a very ſudden and ſharp frigorific proceſs began, which, before the return of day, was ſucceeded by rain. In the beginning and latter part of *December* ſeveral congelations took place, ſome of them keen, but generally moderate; there was a good deal of hazy and foggy weather; and in the middle of the month the air was warm and mellow, producing in the graſs-fields a remarkable verdure. The force of the winds was mo:tly moderate, ſometimes eaſy, and when freſh breezes occurred, they were chiefly in ſhort and ſudden guſts.

The air was remarkably dry during the firſt fourteen days of June, by which the crops ſeemed likely to ſuffer; but fortunately ſome refreſhing rain fell the 15th and 16th, which very much recruited the thirſty vegetation. It was apprehended, that a ſharp north-weſt wind, which blew on the 20th, would have ſpoiled the potatoes; but it was afterwards found that they were very

very little hurt, as the ſtalks were not long enough to ſuffer; and whatever injury the leaves ſuſtained, was repaired by the ſubſequent rain; therefore no worſe conſequence reſulted, than that of retarding their growth a little. In July it was alſo feared that the potatoe crops would not merely ſuffer ſome obſtruction, but that they would in reality be blighted, as on the 4th day of that month the leaves were much blackened by a ſcorching ſouth-weſt wind. The ſtalks, however, eſcaped; and the fine vegetative weather which ſucceeded reſtored verdure even to the leaves. By the drought of Auguſt, it is true, the growth of the potatoes ſeemed to be repreſſed; yet the heavy dews, and a few fogs, appeared to ſupply in ſome meaſure the place of rain: but the flax ſuffered materially; the ſcarcity of water being ſo great as not to leave ſufficient for the operation of ſteeping; ſo that the linen yarn, which was ſpun of this year's flax, has been peculiarly difficult to bleach. The tempeſtuous drift of September, being attended with many fair intervals in the day, was not materially prejudicial to the grain; and the late potatoe crops, which were impeded in their growth by the drought of July and Auguſt, acquired by the rain of this month unexpected vegetation. As in October there were likewiſe many dry intervals in the day with much arefaction in the air, the grain was very little the worſe; and the whole of it, even in the mountainous tracts, was got in on the 20th of the month.

With

With regard to quantity, oats and barley, the chief products of this part of the kingdom, were not defective, except in a very ſmall comparative proportion, and the ſhell contained a full and ſolid kernel. The potatoes, indeed, were deficient in quantity; but the quality of them was generally good. Upon the whole, as to agricultural produce, it may be obſerved, that had not the failure of the preceding year occaſioned a current ſcarcity and a conſumption of ſtores, the year 1800, in this diſtrict, would have rated in its ſupply equal to common favourable years.

About 150 years ago, Dr. Gerard Boate obſerved, that the weather of ſpring in Ireland is generally fair, with clear ſun-ſhine from morning to night, for the ſpace of five or ſix weeks, with little or no interruption, and that this ſet of fair weather begins ſome time in the month of March. On the other hand, he remarks, that our ſummers are generally wet; ſo much ſo that it is a rare thing to ſee a whole week paſs without rain; and many ſummers the weather is never dry two or three days together. With reſpect to the influence of this wetneſs of ſummer on the products of the ſoil, he further remarks, that not only the corn and graſs, but the fruits of trees, ſuffer both in ripening and ſaving.—Yet, he maintains, that the vigilant and careful, who ſeize every opportunity, 'do commonly in the end get in their increaſe well enough, notwithſtanding all theſe great hinderances; ſo that

there be as few years of dearth in Ireland, as in any other country of Chriſtendom; and moſt years there is not only corn enough got for the ſuſtenance of the inhabitants, but a great deal over and above, for the ſending out of great quantities of grains into other countries.'

On this ſtatement I would remark, that the uninterrupted courſe of dry weather, above repreſented as appertaining to our ſpring, neither correſponds with what I have obſerved, nor, as far as I know, with the obſervations of any other writer. The general character of our ſpring weather ſeems to be cold, with frequent dry days, and a conſiderable arefactive power in the air, but with no long abſence of rain, it being rather ſhowery, and April ſo to a proverb. Whilſt Dr. Boate remarks on the wetneſs of our ſummers in general, he admits, 'that ſummers have been, which were *full* of very dry, and fair, and pleaſant weather.' In this repreſentation of certain ſummers, I agree with Dr. Boate; as we often have one remarkably dry ſummer ſeaſon, and ſometimes two or three ſucceſſively, between a greater number of wet ones. He obſerves truly, however, that 'over dry ſummers do in this iſland hardly come once in an age; and it is a common ſaying in Ireland, that the very dryeſt ſummers there never hurt the land.'

Again,

Again, Dr. Boate ſays, it is obſerved 'that ordinarily it raineth in Ireland much more by day than by night; and that, many times, when it doth rain two or three days together, the nights between are very clear and fair.' This kind of weather, he tells us, occurs not only in autumn, but in other bad ſeaſons, when the ſky is overcaſt with clouds and miſts the whole day. As to the firſt branch of the obſervation, my experience leads me to an oppoſite concluſion, having repeatedly noticed, (for example, in the autumn and winter of this year,) that the nights, both in rain and wind, far exceeded the days. As to the ſecond branch, I am not yet prepared to determine how nearly it correſponds with the nature of the weather of late years; but, to the beſt of my recollection, I think that when we have two or three ſucceſſive rainy days, the intermediate nights are fair, or have many fair intervals.

It has been alleged, that, in the inſtance of temperature, the winters of Ireland have been, for ſeveral years paſt, more mild and warm than they were in preceding times, and, conſequently, that in this material circumſtance the climate is altered. But Dr. Boate, bearing teſtimony of the moderation of our winter weather, writes thus: 'It is very ſeldom violently cold there, and freezeth but little: There are commonly three or four froſts in one winter, but they are very ſhort, ſeldom laſting longer than three or four days together,

together, and withal, at their very worſt, nothing near ſo violent as in moſt other countries; ſo that ſome, all winter long, hardly come near a fire in a day; and that not only in the ordinary cold weather, but even whilſt it is a freezing. Yea many times the cold is ſo ſlack, even in the midſt of the winter-months, that by walking only, or doing ſome other moderate exerciſe, you ſhall find yourſelf as warm, and the air as ſweet and pleaſant, as it were in the month of May.'

'THERE hath been ſome winters,' continues he, 'wherein it hath frozen ten or twelve days together, ſo as the Liffie, and other the like rivers, were quite frozen, and might be gone upon by men and beaſts: but thoſe are altogether extraordinary, and do come very ſeldom; hardly once in the ſpace of ten or twelve years. But how mild they ordinarily be, and how little ſubject to exceſſive cold, may appear hereby, that all kind of beaſts and cattle, as cows, horſes, and ſheep, do there all winter long remain abroad, and do feed in the fields, where they are left in the night-time as well as in the day; and that many herbs, which in England and Netherland do dye every winter, here continue all the year long.' Nor was this temperate ſtate of the air, in Boate's time, confined to one ſeaſon of the year, and to the ſouthern provinces; but likewiſe prevailed in every ſeaſon, and in the northern and weſtern diviſions, in ſo remarkable a degree, that, in another paſſage, he aſſures

aſſures us, ‘ the cold was much leſs in the moſt northern part, than any other land lying in the ſame height or latitude, yea than many countries of a much more ſoutherly climate.’ This equality of temperature ſtill continues, as has been proved by late obſervations; from which it appears, that no difference of conſequence takes place throughout the whole iſland; the total variation of heat being only $3\frac{2}{10}$ degrees, which is at the rate of no more than about one degree of temperature for each degree of latitude; a concluſion drawn from experiments on the water of deep wells, for determining the heat of the earth, with which, ſucceeding obſervations ſhew, that the annual medium heat of the atmoſphere nearly correſponds.

One obſervation more from Dr. Boate, I cannot refrain citing, becauſe it relates to the ſalubrity of the air of this iſland, which, if it were as exempt from wet as it is from cold, he proteſts, ‘ would be one of the ſweeteſt and pleaſanteſt in the whole world, and very few countries could be named, that might be compared with Ireland for agreeable temperateneſs.’ To demonſtrate the ſalubrity of the climate, and to ſhew that the healthineſs of the inhabitants is not owing to ‘ any peculiar quality of their bodies, but proceedeth from ſome hidden property of the land and the air itſelf;’ he adduces two facts: ‘ Firſt, in that ſtrangers coming to Ireland do partake of the ſame exemption; and, as long as they continue

continue there, are as free of thoſe evils, from which that climate is exempt, as the Iriſh themſelves.'—' Secondly, in that the natives, born and brought up in Ireland, coming into other countries, are found to be ſubject unto the ſame diſeaſes as other people.' He adds, that even when military bodies, compoſed of ſtrangers, and liable to the ſevere diſeaſes generally incident to an army, were ſeized with ſuch diſeaſes, their origin was traced to errors in the uſe of the non-naturals, not to any peculiar fault in the air of the country.—In what meaſure this laſt obſervation is corroborated by ſucceeding facts, I am not qualified to decide, our data being yet inſufficient for the purpoſe of concluſive calculation; but, if we were allowed to reaſon on the point, we would ſay, That ſince the air was free from the ſpecies of vitiation which, in moſt other countries, occaſions that trying ſickneſs, called *Seaſoning*, at a period ſo far back as one hundred and fifty years ago, when the iſland was in a ſtate vaſtly waſte and uncultivated, compared with its preſent ſituation, may we not now aſcribe to it at leaſt an equally benign diſpoſition, or rather a ſuperior one, ariſing from the improved and ſpreading culture of the land?

It might be worth while to enquire, to what cauſe or cauſes the freſh ſtate of our hibernal vegetation may moſt rationally be aſcribed. Is it excluſively owing to the effect of warmth and moiſture, or to the nature of the ſoil, or to the combination of theſe

theſe qualities? Or has hydrogenous gas any ſhare in it? Warmth, acting in concurrence with moiſture, has unqueſtionably vital influence on the vegetable ſyſtem; but, even in our open winters, cold days frequently occur, the ſolar heat is naturally weak, and moon-light does not appear to poſſeſs any influence deſerving notice in this caſe. And as to the part which the ſoil has in the proceſs, it may be obſerved, that the verdure is not confined to warm and rich grounds, but extends proportionably to every ſoil where herbage exiſts. But it may be further remarked, that in ſome of the colder weather of our verdant winters, the nights, particularly in their beginning, are often ſenſibly warmer than the days. Now, to touch on the borders of hypotheſis, we ſhall aſk, Is the ſuperior temperature of thoſe nights owing to the eſcape of caloric, during the tranſition of water from its mixture with air into a more aggregate ſtate? Taking this queſtion in the affirmative, is it not conceivable that, in the paſſage, ſome of the water might be decompoſed, and its hydrogene, coaleſcing with the free caloric, become hydrogenous gas? However this may be, one thing appears certain, that the atmoſphere is ſometimes ſo highly charged with inflammable gas, that the electric fluid, in regaining its equilibrium, ſets on fire the gas, and occaſions igneous meteors, even very near the ſurface of the earth. (See Note 1,)

The proportion in which inflammable gas contributes to vegetable greenneſs ſeems to be aſcertained by ſome late obſervations. M. Ingenhouſz has remarked, that when this gas is mingled with the air in which vegetables grow, the verdure of the latter acquires a much deeper ſhade. M. Humboldt found that divers plants of the claſs *cryptogamia* puſh out green ſtalks, though placed in mineral pits where light has no acceſs to them; and that ſeveral of the *gramina* have green leaves when growing in the galleries of certain mines, at the depth of 360 feet beneath the ſurface of the earth. On examining the air in theſe ſituations, he found that it was mingled with hydrogenous gas. Theſe curious phenomena, which M. Humboldt employs in a ſatisfactory manner to explain the *etiolation* (blanching) of plants, tend equally to render it probable that this gas has at leaſt ſome ſhare in giving the ſoft-toned green, the delightful *colour of nature*, to the vegetable creation. As rain-water, however, is found to contain, in a ſtate of ſolution (not in combination like ſnow) a portion of oxygene, and as the contact of oxygene expedites the germination of ſeeds, a ſhare in producing the verdure of our winters may of courſe devolve on atmoſpherical moiſture and warmth. But I queſtion whether to theſe cauſes can be legitimately aſcribed the whole of the verdure, as after the diſſolution of deep ſnow, (which is more oxygenated than rain-water, and which, being a bad conductor of caloric, confines the atmoſpherical heat,) the greenneſs of plants is much paler,

paler, and their growth far leſs advanced, than in an open winter. (See Note 2.)

In accounting for the temperature of our winter air, perhaps we ſhould attribute ſomething to the geographical poſition of Ireland with reſpect to the Atlantic, and the comparative ratio of its altitude. From the Atlantic we have the warming effects of the gulf-ſtream more powerfully than they can have it in the northern iſles of Britain, as it muſt ſuffer a decrement of heat before it can arrive at their coaſts, which it reaches with a very weak velocity about Chriſtmas. The lowneſs of our lands, alſo, compared with other countries, even in Europe, is very ſtriking. The higheſt part of the *general* ſurface of Ireland, which is only 270 feet above the level of the ſea, is about the center of the kingdom; and our loftieſt mountain, M'Gillicuddy's Reeks, in the county of Kerry, riſes but 3405 feet. Whereas, taking other countries in gradation, Paris is 365 feet over the ſea; Geneva, 1268 feet above the Mediterranean; the Valley of Chamouni, 3367 feet over ditto; Snowdon, in Wales, 3555 feet over the ſea; Benevis, in Scotland, (the higheſt hill in Britain) 4370 feet; the inn on St. Gothard, in the Alps, 8819 feet nearly; Peak of Teneriffe, 12185 feet; Mount Blanc, (the higheſt land in the old world) 15662 feet; and that gigantic mountain-maſs, Chiromboraco, in Peru, 20595 feet above the ſea. (See Note 3.)

From the above comparative ſketch of the climate of Ireland, in the middle of the 17th century, with its conſtitution for a few years paſt, it will appear, that advantages might be derived to agricultural ſcience from a yet more extended compariſon. But this is not all; another very important benefit might be reaped from ſuch a compariſon, in a medical point of view. Although I do not believe that, in theſe latitudes, health, or diſeaſe, 'follow ſtill the motions of the moon,' yet I am firmly perſuaded, that theſe circumſtances of the human body are materially influenced by the conditions of the element in which we dwell, and feed, and have our being. Indolence, ignorance, and other cauſes have led men to neglect, or deſpiſe inveſtigations concerning the application of meteorology to medicine; and, perhaps, no cauſe has contributed to this effect more than the want of comparative eſtimates. During many ages, it was the faſhion to regard the obſervations which had been made on thoſe ſubjects in Greece and Rome, as applicable to the air and inhabitants of countries in ſeveral reſpects differing from theſe and from one another; and meeting with diſappointments in transferring the rules of what was called the ſtandard ſituation to other places, the inquirers have given up the purſuit as vain and uſeleſs. But had a ſufficiently extended compariſon been accurately made between the air and inhabitants of the reſpective countries, each inquirer would probably have learned an inſtructive leſſon concerning the climate and diſeaſes of his own

diſtrict.

diſtrict. And theſe kinds of obſervations, multiplied in different regions, would in proceſs of time accumulate ſuch a maſs of facts as would furniſh materials for determining a very intereſting queſtion: *Is meteorology applicable to the ſcience and art of medicine?*

That the phenomena of the atmoſphere, like all the other works of nature, notwithſtanding the viciſſitudes and apparent irregularities of the weather, are governed by certain laws, cannot be for a moment doubted; and, that they all have their phyſical cauſes, we are not authoriſed to deny, merely becauſe we cannot diſcover them. Thoſe phenomena, unqueſtionably, ariſe from ſome providential ordinances of which we are at preſent ignorant, and which nature follows as conſtantly as ſhe does thoſe with which we are acquainted. If this was not the caſe, we muſt roundly aſſert, that there are actions which have no principle, and effects which have no cauſe; aſſertions totally repugnant to all philoſophy. Why, then, may we not, by patient inveſtigation, diſcover a clew to detect the meanders of thoſe phenomena? Why may we not be capable of tracing their laws? Why may we not ultimately be able to form theſe laws into a code, and reduce meteorology, already putting on an improved aſpect, into a ſtill more ſcientific ſyſtem, applicable, like chemiſtry and other ſciences, to both the ornamental and uſeful arts?

How many and great diſcoveries have been made in the walks and operations of nature, which previouſly were not even dreamed of, or, if they were conceived, were deemed viſionary and impoſſible! Should we, therefore, be diſcouraged in one branch of natural knowledge, not perhaps ſo intricate and forbidding as ſome which have been ſatisfactorily explored? But what method might be taken to cultivate this branch in Ireland? Might it not come within the province of the Dublin Society to commence and carry on an undertaking of this deſcription, by attaching to their inſtitution a profeſſorſhip of meteorology, on the ſame wiſe and enlarged principle that they have eſtabliſhed one of botany and another of chemiſtry and mineralogy. And is it not reaſonable to preſume, that the legiſlature would make proviſion for the ſmall expenſe it would require, when the advantages to be derived from it might be ſo extenſive and important?

'THE neceſſity of ſome public eſtabliſhment for meteorological inquiries,' was ſeveral years ago pronounced by an authority, which no friend to this country and to ſcience will think of diſputing. In a general view, ſuch a foundation evidently might be rendered productive of much additional knowledge; and in a particular inſtance, namely, the medical department, many new and intereſting materials might be furniſhed, paths yet untrodden might be explored, and great advances might be made, in a ſhort time, toward

toward deciding the influential queſtion, before noted, concerning the application of meteorology to the healing art. In fine, for an inſtitution of this kind, it is judiciouſly remarked, (Tranſ. R. I. Academy, Vol. II.) that Ireland is peculiarly well circumſtanced. 'The local diſpoſition of our iſland, conſiderably advanced in the weſtern ocean beyond the other ſhores of Europe, and removed from the immediate influence of any great tract of mountainous country, renders it peculiarly fitted for a *ſtandard* ſituation, to which the obſervations of other countries may be conveniently referred.' Seeing, then, that Ireland is thus adapted by nature for the propoſed reſearch, at once curious and uſeful, it would be a pity, ſurely, to neglect cultivating its advantages with ſuitable zeal and aſſiduity. The Academy, indeed, rightly impreſſed with the truth of this opinion, early made it an object of their attention; but it appears, that the chief meaſure which they took on the occaſion, (and which ſeems to have been the utmoſt that could have been expected from them) viz. the diſtribution of inſtruments, has been followed with ſuch inadequate ſucceſs, as to leave room for wiſhing that ſome effectual plan might be adopted. It ſeems neceſſary that there ſhould be a central ſource, which, like the heart in the human body, might diſpenſe its vital fluid in animating currents throughout the nation.

(NOTE 1.)

(NOTE 1.)

We may here note, that phoſphoriſed hydrogene gas (a diſcovery aſcribed to C. Gengembre) poſſeſſes the ſingular property of inflaming by the contact of air alone, without requiring its temperature to be raiſed, or an ignited body to be preſented to it. But whether this ſpecies of gas is capable of communicating the green colour to vegetables, and whether it is generated in the atmoſphere, or earth, are queſtions to which I am not here prepared to give anſwers of a ſatisfactory nature.

(NOTE 2.)

The ancient Greeks aſcribed the green colour of vegetables to the influence of ſolar light; but they explained this opinion by a theory, the ſubſtance of which is as follows: 'There are but three ſimple colours, white, black, and yellow. Theſe depend on the elements: white, on the air, water, and earth; yellow, on fire, or the inflammable principle; black, on a want of light. The mixture of the elements may occaſion different colours. Thus from the conjunction of water with the rays of the ſun, vegetables acquire a green: when earth and water co-operate without the influence of light, they aſſume only white, whilſt thoſe parts of the plant which riſe out of the earth are green.' Hence it appears, that the Greeks had, on this ſubject, ideas ſimilar to thoſe which are ſupported by Meſſrs. Ingenhouſz, Sennebier, Humboldt, &c. Here then we have cauſe to aſk, in the words of the eloquent Preacher, pronounced near 2800 years ago: 'Is there *any* thing whereof it may be ſaid, See, *this* is *new*?'

(NOTE 3.)

(NOTE 3.)

The higheſt point of the *general* ſurface of Ireland is taken from obſervations and ſurveys made to find the elevation of the ſūmmit level preparatory to cutting the Dublin canals. The altitude of the Kerry mountain was accurately meaſured by Mr. Kirwan. And the foreign altitudes are given on the authority of La Lande, Sir G. Schuckburgh, General Roy, Chevalier Verdun, Sauſſure, and Bouguer.

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ERRATA.

Page 304, In Table the 2d, in the line of Reaumur's Thermometer, *for* 10°. 9°. 8°. 7°. 6°. 5°. 4°. 3° 2°. 1°. *read them*—10°—9°—8°—7°—6°—5°—4°—3°—2°—1°.

—— 305, —— 5, from top, *for* are determined, *read* be determined.

—— 310, —— 10, from do. *for* complement of 9°,5, *read* complement of—9°,5.

—— 311, —— 1, from do. *for* complement of 9°,5, *read* complement of—9°,5.

—— 317, —— 6, from do. *for* 1799, *read* 1790.

———— —— 8, from do. *dele the point after* foil *and add* a comma.

—— 319, —— 3, from bottom, *for* 582,175 grs. *read* 582175 grs.

—— 339, In the fixth column, and *under* the Expanfions of one Inch at 32°, line 8 from top, *for* ,1250909, *read* ,1250900; and line 9, fame column, *for* ,1295557, *read* ,1295575.

—— 340, —— 6, from top, *for* D'Amontoms, *read* D'Amontons.

—— 344, —— 5, from do. *for* Tremly, *read* Trembley.

—— 345, —— 6, from bottom, *for* $\frac{1}{42}$, *read* $\frac{1}{144}$.

—— 365, In the fourth column, and 2d line from top, *under* above by obfervation, *for* 5°, *read* 45°.

—— 373, —— 6, from top, *for* mole, *read* Mole.

—— 407, —— 1, at ditto, *for* SECT. VI. *read* SECT. V.

—— 409, In the fourth column, and 1ft line, *under* Mathem. *for* 80,7°, *read* 80°,7.

—— 413, —— 3, from top, *for* of the hemifphere, *read* as the northern hemifphere.

—— 417, —— 1, at top, *for* SECT. VII. *read* SECT. VI.

—— 418, —— 5, from top of the note, *add* after beyond which, the increafe of.

———— —— 8, ditto of note, *for* itt maximum, *read* its maximum.

—— 429, —— 2, from bottom, *for* 328, *read* 32°.

—— 441, —— 6, from ditto, *for* manomeeers, *read* manometers.

—— 446, —— 5, from top, *for* 2,44 × 36, *read* 2,43 × 36.

—— 491, —— 7, from ditto, *for* hitherto, *read* heretofore.

—— 493, —— 14, from top, *for* neceffaty, *read* neceffary.

—— 500, —— 1, at ditto, for Hydrometer, read Hygrometer.

Of the VARIATIONS *of the* ATMOSPHERE.

By RICHARD KIRWAN, *Esq; L.L.D. F.R.S. and P.R.I.A.*

Read May 11, 1801.

THE variations of the known properties of the atmoſphere, viz. of its ſtate of moiſture or dryneſs, and its diſpoſition to admit, retain, or diſmiſs the watery fluid, its temperature, gravity, elaſticity, and electricity in different ſeaſons, and frequently in the ſame ſeaſon, and in different years, in the ſame or in different latitudes, at the ſame or at different diſtances, whether vertical or lateral, from the oceans ſeparating the two continents that form the ſolid maſs of the globe, as well as on thoſe oceans themſelves, and the currents which from different points with more or leſs permanency agitate the whole atmoſpheric maſs, form, together with the inveſtigation of their cauſes, the principal objects of meteorological ſcience.

Though this ſcience has been cultivated with much ardour in many parts of Europe ever ſince the diſcovery of the gravity of the fluid that ſurrounds us, it muſt be owned very little progreſs has hitherto been made in it. In every other branch of phyſical ſcience from a knowledge of the cauſes we are enabled to foreſee the effects; but from a knowledge of the actual ſtate of the above mentioned properties of the atmoſphere, who can with certainty foreſee what it ſhall be the next day?

The obſtacles which originally obſtructed all advances in this ſcience were,

1°. The want of proper inſtruments to meaſure the proportion or quantities, whether of moiſture in the atmoſphere, or of its weight, or its temperature, its denſity or its electricity.

2°. The want of general ſtandards to which, as to their proper units, the ſeveral degrees of each property may be referred.

3°. Interruption of the obſervations of the ſtates of the atmoſphere, by reaſon of which their connexion muſt remain unknown.

4°. As the actual ſtates of the atmoſphere are intimately connected, not only with the preceding, but with the contemporary ſtates in different and moſt diſtant parts at leaſt of the ſame hemiſphere, the neglect of comparing either of them, as far as they

they were or might be known, has been, and is perhaps at preſent, the principal ſource of our ignorance of the cauſes that produce, vary, or interrupt them. Moſt meteorologiſts content themſelves with noting the diurnal ſtate of the atmoſphere in the place of their reſidence, without any referenee to its ſtate in diſtant countries; and thus, for want of concert, *dum ſinguli pugnant omnes vincuntur.*

5°. The neglect of obſerving the ſuperior currents of the atmoſphere, which may at preſent be detected by tracing the direction of baloons, in all ſeaſons.

6°. The neglect of maritime obſervations, which, being freeſt from all local influences, are by far the moſt inſtructive, and ſhould, if poſſible, be made in all degrees of longitude and latitude.

However, many of theſe obſtacles have of late been happily removed. The proportion of moiſture in the atmoſphere relatively to its ſaturability, in its various ſtates, whether of denſity or temperature, can now be perfectly aſcertained by the hygrometer of the late excellent Mr. De Sauſſure, who has by the moſt extraordinary efforts of genius and labour improved that inſtrument, ſo as to make it ſpeak a language every where uniform and intelligible; a degree of perfection ever before thought unattainable. Nay, by a number of moſt ingenious and delicate

experiments, he has been enabled to make it indicate very nearly the abſolute quantities of moiſture contained in given cubic meaſures of atmoſpheric air of given denſities and temperatures. Of this inſtrument and its uſes, as Mr. Sauſſure's treatiſe has never been tranſlated into our language, and ſeems unknown to our meteorologiſts, I ſhall give an abridged account in the following pages.

Mr. De Luc has alſo beſtowed much laudable induſtry in perfecting an inſtrument, applicable to the ſame purpoſes, but leſs fragile; how far he has ſucceeded I am not enabled to ſay, not being poſſeſſed of it.

Barometers have been brought to the utmoſt degree of perfection by the ſagacity and unremitting labours of Mr. De Luc, as may be ſeen in his immortal work on the modifications of the atmoſphere. The varying denſities of the atmoſphere at different heights (denied by Buffon) have been ſatisfactorily proved by the numerous and happily contrived experiments of Meſſrs. Sauſſure the father and ſon. See 36 Roz. p. 98.

Thermometers have long ſince been improved by Fahrenheit and Reaumur; but the imperfections and uncertainties attending many under this laſt denomination have been detected by Mr. De Luc, who in their room introduced a much more perfect inſtrument

ment which ought to bear his name. Yet the inconveniences attending the variations of the ſcale have not been ſufficiently felt by ſome ſubſequent philoſophers, who thus unhappily, and to no purpoſe, multiply the labours of the meteorologiſt. Mr. Van Swinden's excellent treatiſe on thermometers enables us to underſtand the diſcordant languages of many of them. And of late centeſimal thermometers (the ſame as the Swediſh) have been introduced in France. The degree of cold at which mercury freezes was determined by Mr. Cavendiſh from the experiments of Mr. Hutchins.

Electrometers of the leaſt fallacious kind have been deviſed by Mr. Cavallo.

Of *Anemometers*, though many have been invented to denote the direction and the force of winds, yet none of them has, as yet, been introduced into general uſe. Mr. Smeaton's table of the rapidity and force of the winds, Phil. Tranſactions 1759, ſeems the beſt that has as yet appeared.

2° With reſpect to *ſtandards*, Sir George Schuckburg has ſhewn, from 132 obſervations made in Italy and in England, that the mean height of the barometer at the level of the ſea, the temperature of the mercury being 55° and of the air 62°, is 30,04 inches,

inches, Phil. Trans. 1777, p. 586. We may then aſſume the height of 30 inches as the natural mean height of the barometer at the level of the ſea in moſt temperatures between 32° and 82°; for if the mercury were cooled down to 32°, that is 23° below 55°, it would be lowered by that condenſation only 0,07 of an inch; and if it were heated up to 80°, that is 25° above 55°, it would be raiſed only ,078 of an inch; quantities which except in levelling may be ſafely diſregarded.

The French have heretofore conſidered 28 Paris inches as the mean height of the barometer at the level of the ſea, that is 29,84 Engliſh inches. But from 1400 obſervations made at Rochelle by Fleaurieau De Bellvue, and from five years obſervations made at Port Louis in the Iſle of France, he concludes the mean height of the barometer at the level of the ſea to be 28 inches and 2 lines and $\frac{10}{12}$ of a line in the temperatures of from 52° to 55° Fahr. = 30,08 Engliſh inches, 4 New Roz. p. 159*. Hence we may conſider in round numbers 30 inches as the ſtandard height of barometers at the level of the ſea. And knowing the true height of any part of the earth, we may by ſubtracting that height expreſſed in fathoms from the log. of 30, viz. 4771,21, find the logarithm which indicates the number of inches at which, as

* La Chappe thought it 28 inches 1,5 lines. See Beguelin's Memoir, Mem. Berl. 1769. 12 Coll. Acad. 424.

as its natural mean, the mercury ſhould ſtand at that height over the level of the ſea.

THUS ſuppoſing the height to be 87 feet, equal 14,50 fathom, then 4771,21—14,50 = 4756,71, which is the logarithm of 29,9; this therefore is the natural mean height of the barometer at the elevation of 87 feet over the level of the ſea.

CONSEQUENTLY to all the heights heretofore, calculated by the French over the level of the ſea, 139,32 feet muſt be added, Engliſh meaſure, when the Mercurial height at the level of the ſea was barely ſuppoſed to be 28 French inches.

WITH reſpect to *temperatures*, I have eighteen years ago indicated a ſtandard (none having been before thought of) with which the temperature of all the inhabited parts of the globe, whoſe latitudes are given, may be compared. La Cotte, whoſe unceaſing labours contribute ſo largely to the progreſs of meteorological knowledge, has ſince extended this compariſon to 200 towns in 30 different degrees of latitude, and found their mean temperature to coincide with my eſtimation twelve times. That the greateſt error was 6,75 degrees of Fahr. which occurred four times, and the mean error to be 0,36 of a degree of Fahren. 37 Roz. p. 412. Such errors, not in the principles but in their application, might

might well be expected where the mean temperature of only a few years obſervations could be procured. On this occaſion I cannot forbear remarking, that, in calculating the mean temperature of places on the obſervations of a few, or even of many years, the extremes, whether of heat or cold that happen not above once in ten years or ſtill more rarely, ſhould be omitted and only mentioned in a note.

The third obſtacle, viz. the interruption of obſervations, and alſo the 4th, 5th and 6th, cannot be remedied but by the eſtabliſhment of meteorological ſocieties in different parts of Europe, annually communicating with each other (ſo liberally endowed as to be enabled to employ proper obſervers at proper diſtances), and obliged to publiſh annual accounts of their ſeveral obſervations, and to compare them with thoſe made in foreign countries. When the vaſt importance of meteorological knowledge to agriculture and navigation is conſidered, there is at preſent ſome room to hope that eſtabliſhments of this nature may take place at no very diſtant period.

The number of perſons, who, from a view of attaining a ſettlement in ſuch eſtabliſhments, would receive an education in the different ſciences connected with meteorology, (that is indeed all the different branches of mathematics and natural knowledge), and

and the emulation, thus excited, would neceſſarily diffuſe a conſiderable maſs of knowledge throughout the community.

THEODORE, the late Elector Palatine of the Rhine, realized this project in his dominions; and that incomparable ſovereign, *Catharine the Great*, entertained the ſame deſign ſhortly before her deceaſe.

SOME other petty impediments ſtill remain, which unqueſtionably occaſion a great loſs of time, but which meteorologiſts themſelves may eaſily remove, namely, the diſcordant notation of their inſtruments, and the uſe of duodecimal, nay even in ſome caſes, of ſexageſimal diviſions, inſtead of the decimal, which is ſo much more commodious, and the application of the vague terms, *hot*, *cold*, *wet*, *dry*, to certain periods, without any deſignation of their preciſe meaning.

AMONG the moſt zealous promoters of meteorology in the laſt century we find ſome of the moſt eminent philoſophers it produced, Derham, Halley, De la Hire, Mairan, Reaumur, Wargentin, Leche, Kraft, Braun, Richman, Sauſſure, Beguelin, &c. and among thoſe now living, De Luc, Doctor Horſeley, Van Swinden, Cotte, Monge, &c.

THE late great navigator Captain Cooke, and the ever to be lamented La Peyrouſe, Mr. Dalrymple, and Captain Phipps, have

materially benefited this ſcience by regiſtring the variations of their barometers and thermometers during their voyages.

Of the Moiſture of the Atmoſphere.

Moisture may be conſidered as riſing into the atmoſphere, or ſubſiſting in it, or deſcending from it. I ſhall firſt conſider its aſcent.

CHAP. I.

Of Evaporation.

Evaporation is the converſion of a liquid (and frequently even of a ſolid) into an inviſible fluid. That the liquid is diminiſhed is evident from its loſs of weight; but that the aqueous liquid, which I have here principally in view, is not decompoſed, is equally clear, ſince the inviſible fluid, into which it is converted, may be made to re-appear in its former viſible ſtate.

This converſion may be effected either naturally or artificially: that produced by nature is always accompanied by contact with the atmoſphere; the artificial is producible in contact with the atmoſphere, or even in *vacuo*, as is now well known.

In the common courſe of nature five cauſes concur in producing evaporation: heat, affinity to atmoſpheric air, agitation, electricity, and light.

Section

Section I.

Of the Influence of Heat.

The connexion between heat and evaporation is too obvious to have eſcaped the notice of even the moſt inattentive to the operations of natural cauſes. Every one knows that, in general, the greater the heat applied to water in ſimilar circumſtances, the more quickly it will be converted into vapour. As aqueous vapours are by far the moſt abundant in the atmoſphere, to their conſideration I chiefly attend in this Eſſay. I have ſaid *in general*, becauſe Leidenfroſt's experiments, and ſome others which will be mentioned in the ſequel, ſhew that this law muſt be circumſcribed within certain limits.

That all liquids are expanded by an increaſe of heat is alſo well known. This rarefaction is oppoſed by two powers, namely, the attraction of their integrant molecules to each other, and that of external preſſure. But there is ſcarce any degree of heat ſo low, as to be totally diſabled to overcome the reſiſtance oppoſed by the power of integrant attraction; as appears by various experiments, in which, external compreſſion being diminiſhed or withdrawn, the rarefaction and evaporation could ariſe from heat ſingly. Thus Sauſſure found that water is more readily converted into

vapour *in vacuo*, or in rarified air, than in air of greater denſity; becauſe, as he well remarks, all external preſſure reſiſts the expanſion of water into vapour, *Hygrom.* 219. It is true that he adds, in the ſubſequent page, that, in a given time, water loſes more of its weight by expoſure to the open air than it does *in vacuo*; but to prove this fact he only quotes the experiment mentioned in the 55th vol of the Philoſophical Tranſactions, p. 146*, where indeed it is ſtated, that a cup of water ſtanding under an exhauſted receiver, loſt only 2,5 grains in 24 hours, whereas the ſame quantity of water expoſed to the open air during that period of time, loſt 35 grains. But it is plain that, even ſuppoſing the temperature and ſurfaces of the water in both cups equal, the reſiduary air under the receiver (whoſe capacity is not mentioned) muſt have been ſaturated long before that in the room could have been ſo, and thus its evaporation ſpeedily checked. It is expreſsly ſaid that the air under the receiver was only 43 times rarefied; enough therefore remained to be ſoon ſaturated: but if it had been ever ſo perfectly exhauſted, the elaſticity of the vapour already raiſed would have been ſufficient to check farther evaporation. Beſides, ſince it is acknowledged that water evaporates more eaſily *in vacuo* than in open air, it neceſſarily follows that, if the temperature remained the ſame, and if the vapours had liberty to eſcape, it would alſo evaporate more copiouſly. The quantities evaporated in ſmall portions of time are thoſe that ſhould be compared with each other;

* By miſtake he quotes the 59th vol.

other: this I have done, and found the loſs *in vacuo* incomparably greater than that in open air; the difference however I cannot now ſtate, having loſt the note I took of it. Mr. Watt has ſhewn, that water boils *in vacuo* at a degree of heat little ſuperior to that of the human body, and diſtills as faſt as it does in open air at 212°, 1 Mem. Mancheſter, 390. Water has alſo been diſtilled *in vacuo* by Wallerius, 11 Collect. Acad. 166, &c. and by the immortal Lavoiſier, Mem. Par. 1770. Hence in nature water evaporates more when the barometer is low than when it ſtands high, and conſequently diſtillation may be carried on on the tops of mountains more readily, and with leſs heat, than at the level of the ſea.

According to Mr. Cavendiſh, water heated to 72° is not (ſuddenly) converted into vapour under an exhauſted receiver, until the preſſure is no greater than about $\frac{1}{40}$ of the uſual preſſure of the aſmoſphere, that is about three-fourths of an inch; and if the water be heated only to 41, the preſſure muſt be reduced to one-half an inch, but the vapours thus raiſed are immediately turned back into water on reſtoring the preſſure of the atmoſphere, Phil. Tranſ. 1777, 622. But it is plain that he means, that the greater part only is re-converted into vapour (unleſs the air is already ſaturated) by the increaſed preſſure; for he allows that water expoſed to the open air will evaporate at any

any degree of heat, and under any preſſure; and this he attributes to the action of air upon it. And ſince the removal of the greater part of the preſſure promotes evaporation, the diminution of a ſmall part of the preſſure muſt promote it in a degree proportionably ſmaller, let the temperature be what it may. Hence vapours ariſe more copiouſly and readily in rarefied air than in air of greater denſity, if the temperature of both ſorts of air be equal.

The conſequence deduced from theſe experiments is farther ſtrengthened by the obſervations of Sauſſure on evaporation on the Col de Geant compared with that which took place at the ſame time at Geneva. On the mountain the barometer ſtood at 19,88 Engliſh inches, and at Geneva at 29,04. The thermometer on the mountain was at 45°,3 of Fahr. at a mean, and at Geneva at 50°, yet on the mountain 39,5 grains were evaporated, while at Geneva only 14,5 grains diſappeared. It is true that the air at Geneva wanted only 2,38 grains to ſaturate it, while that on the mountain wanted 4,03; but ſo ſmall a difference cannot account for ſo conſiderable a diſparity in the effect. See the ingenious calculations and the ſeries of conſequences deduced by this excellent philoſopher, *Voyages aux Alpes*, vol. vii. 8vo. § 2059 and 2060. His general concluſion is, that a diminution of the denſity of the atmoſphere amounting to one-third, every thing elſe being equal, doubles the quantity of evaporation, § 2062.

Hence

Hence Mr. De Luc and ſome other philoſophers conclude, that the affinity of air to water, if any, no way contributes to evaporation, but that it is the mere reſult of the abſorption of caloric by water at its ſurface, which abſorption renders a portion of it ſpecifically lighter than the air in contact with it, *Idees ſur la Meteorol.* § 2, &c. The denſity, to which vapours thus raiſed are capable of arriving, hath certain *limits*, each relative to their temperature, being more extenſive in higher temperatures, and narrower in low temperatures. The ſpecific gravity of vapour he conſiders as leſs than that of air by more than one-half, their maſs being above one-half leſs than the maſs of an equal volume of air poſſeſſing the ſame elaſticity in the ſame circumſtances, ibid. § 7. Again, if, ſays he, theſe vapours ſubſiſt in any ſpace, unmixed with air, in that caſe any preſſure, ſuperior to the expanſive force which they poſſeſs in a given temperature, will, by forcing their particles into cloſer contact, oblige them to unite, and thus partially condenſe them, or (as he, in my opinion, improperly calls it) decompoſe them; but if any quantity of air, capable of ſupporting this exceſs of preſſure over their expanſive force, be mixed with them, they will ſupport their vaporous ſtate, § 14 and 554.

I muſt confeſs that the commonly received doctrine of the ſolution of vapours in air, at leaſt in air of the uſual denſity, on

plains,

plains, appears to me much more intelligible and better founded. In this ſyſtem, firſt ſuggeſted by Halley, and beautifully illuſtrated by Roy, and ſtill farther extended by Doctor Hamilton, water is attracted by air, and forms a compound lighter than *mere* air of the ſame temperature, and under the ſame preſſure; but the difference of their gravities is not conſiderable. Whereas, in Mr. De Luc's hypotheſis, water and air, not being chemically united, do not form a compound, but rather what in technical language is called an aggregate, like water and oil mixed together; if ſo, vapours, being ſpecifically lighter by one-half than air of the ſame temperature, ſhould riſe above the ſtratum of air in which they are formed, until they are cooled during their aſcent, or meet with air of the ſame denſity, betwixt which and air of ſuperior denſity they muſt remain. If ſo, vapours ſhould never be found in the lower ſtrata of the atmoſphere, unleſs they be perpetually renewed; conſequently, if the ſource of evaporation be cut off, as it is when moiſt air is placed under a receiver ſtanding on metal, or on any other dry ſubſtance, and the temperature be kept undiminiſhed, the vapour originally diſperſed through the whole of its volume ſhould gradually collect and float at the top, as happens when water and oil are mixed together.

But this ſeparation of air and vapour never happens. For if concentrated ſulphuric acid, or dry cauſtic tartarin, or muriated lime,

lime, be, after ſome days, carefully introduced under the ſame receiver, all the moiſture will, after a few days, be extracted by them, let the receiver be ever ſo tall, as I myſelf and others have frequently experienced; which could not happen if theſe ſubſtances were not in contact with the vapours, which are perpetually drawn down by the attraction of the lower portions of air as faſt as they are deſiccated, and thus the contact perpetually kept up. Moreover, cold is known to be produced in air by evaporation, but cold is never produced by mixture, but it often is by ſolution. Many other arguments might be adduced in proof of the ſolution of vapours in air, but theſe appear to me ſufficiently convincing. See alſo Schmidt's excellent treatiſe on vapours in 4 Gren's Jour.

Yet it muſt be remarked, that though air favours the riſe of vapour by its affinity, yet it oppoſes its formation by its preſſure, and, conſequently, does not contribute to its origin, but only receives it in its naſcent ſtate, and diſperſes it through its own volume, and enables it to ſupport a degree of cold, greater than it could bear if ſingle; and as its preſſure is proportioned to its denſity, denſe air oppoſes evaporation more than rarefied air does, as already ſeen.

But denſe air requires a greater quantity of vapour to ſaturate it than rarefied air, and, therefore, in equal volumes it may con-

tain a greater quantity of moiſture than rarer air, and in all caſes air muſt have, to admit vapour, liberty of expanſion; for in veſſels full of air, and perfectly cloſed and reſiſting dilatation, no more vapour can riſe but as much as ſaturates that air; and, conſequently, diſtillation cannot take place, let ever ſo much heat be applied, as Fontana has ſhewn, 13 Roz. Journ. p. 22.

Doctor Eason, in 1 Mem. Mancheſt. p. 396, argues, that if vapours were diſſolved in air, the air that receives them would have its weight increaſed by the additional weight of the vapours; but it is known, that moiſt air is lighter than dry air. This is certainly true, but then it is only ſpecifically lighter than dry air. And it is well known that many, even metallic, alloys, are ſpecifically lighter than either of their ingredients. Air receiving vapours acquires a ſtill greater addition to its volume than it does to its weight; beſides, vapour is lighter than air, not indeed 1800 times, for that is true only of *ſteam*, (the vapour of boiling water) not of common vapour, as *De Luc* himſelf now allows.

The Doctor adds, that if the doctrine of ſolution were true, we could not have rain unleſs the air were ſuperſaturated with water, which is true with reſpect to ſome or other of the *ſtrata* of the atmoſphere, for rain proceeds from clouds, and clouds conſiſt of vapours

vapours which had paſſed the limits of ſaturation, and therefore have, in ſome degree, coaleſced; but their farther coaleſcence into rain is owing to electrical agency, as the Doctor himſelf well explains in the ſucceeding pages.

The evaporation of water expoſed to the air is encreaſed or diminiſhed by various circumſtances, ſome relative to the water and ſome relative to the air. Thoſe relative to the water are, 1°. its temperature; 2°. its ſurface; and, 3°. its purity.

And 1°. as to *temperature*. It appears from Lambert's experiments made, in a room in which there was a ſtove whoſe heat could be regulated, that evaporation proceeds in a different ratio from our artificial degrees. The reſults are exhibited in the following table:

Heat of the water.	Ratio of Evaporation.
54°,5	1
77,	3
99,5	6,5
124,	12,
144,	20,5
167,	32,5

The degrees of heat were taken by Lambert on Reaumur's ſpirit thermometer, and the evaporation eſtimated by lines of the Paris

 inch,

inch, of which I have given the ratio, and have converted Reaumur's into Fahrenheit's ſcale. It appears that between 54°,5 and 99°,5 evaporation is nearly proportional to the differences of the temperature of the water, for as 22,5 (the difference between 54°,5 and 77°) is to 3, ſo 45, (the difference between 54°,5 and 99°,5) is to 6.

From a ſeries of experiments which I proſecuted in London during the year 1785, I am inclined to think that, other circumſtances being equal, the ratios of evaporation to the temperature of water, are as expreſſed in the following table:

Heat of Water.	Evaporation,
32°	1 Grain
45	2,5
55	4,
65	5,
75	6,5

These experiments were made on water contained in a cylindrical glaſs veſſel, under a ſurface of 25,23 ſquare inches, and containing 23,13 cubic inches. It was partially expoſed to the wind and air between the hours of twelve and two o'clock in the afternoon, but ſheltered from rain. In winter the initial heat of the water was generally a few degrees higher than that of the air,

air, and in ſummer a few degrees lower; but of the variable influences of the winds, or of the ſaturability of the air, I can give no account, except that of this laſt I may ſay, that the hygrometer never ſtood lower than 70°; but as theſe influences ſometimes concurred with, and ſometimes oppoſed, the action of heat, it is probable that, in producing the general effect collected from a number of experiments, they balanced each other.

2°. With reſpect to *ſurface*, it is now generally agreed, in conſequence of the numerous and accurate experiments of Wallerius and Richman, repeated and confirmed by Lambert, that other circumſtances relative to the air and to the water being alike, evaporation, in equal *times*, is exactly in the ratio of the extent of the ſurface which water expoſes to the atmoſphere; and hence it is, that agitation of the water, by increaſing and varying the ſurfaces, promotes evaporation. Yet Muſchenbrock, in his notes on the experiments of the Academy *del Cimento*, relates ſome, in which he found, as he imagined, that the depth of water concurred with the ſurface in promoting evaporation; ſo that if the ſurfaces were equal, the evaporation would be as the cube-roots of the depths; thus evaporation, from a veſſel eight inches deep, and filled with water, would be twice greater, than the evaporation from another veſſel of equal ſurface in the aperture, but only one inch in depth. In this experiment both veſſels were of lead,

lead, and placed near each other, three feet above the ſurface of the earth, and expoſed to the full action of the air, wind and ſun. 1 Collect. Acad. p. 142. But he adds, that when both were placed in a room, within his houſe, the daily evaporation from each was equal.

In the firſt experiment, a diſparity of temperature ſeems to me to have taken place during its continuance, which occaſioned the inequality of evaporation. The external air conſtantly varies its temperature: now ſuppoſing the initial temperature of both veſſels and of the water contained in them to be equal; and ſuppoſing the external air ſome degrees hotter or colder than the water in theſe veſſels, the ſmaller of the two veſſels, and alſo the water contained in it, muſt have acquired the temperature of the air ſooner than the water in the larger of the two, and thus its evaporation muſt ſooner have been checked, as will preſently be ſhewn in treating of the influence of the temperature of the air, and this check to evaporation muſt have been renewed after every change in the temperature of the air; whereas, the water in the larger veſſel muſt have, during each change, acquired the temperature of the air more ſlowly, a circumſtance which promotes evaporation, as will be ſhewn. Therefore, in the ſecond experiment within doors, where the variations of the temperature of the air were fewer, or none, the daily evaporation from each veſſel was nearly equal.

And

And thus we ſee, that depth is a circumſtance of no importance, except as far as it is connected with temperature. But if we ſuppoſe, during the progreſs of the evaporation, ſuch a change in the temperature of the air as reduces it to an equality with that actually exiſting in the larger veſſel, and below that which already took place in the ſmaller, (which may happen by the intervention of a cloud) in that caſe the evaporation of the larger will be checked, while that in the ſmaller goes on. And thus we may explain ſome ſeeming anomalies in the experiments of Wallerius.

This celebrated naturaliſt expoſed to the open air, in the month of Auguſt, two tin parallelopipeds filled with water, whoſe ſurfaces were nearly equal, but the height of one of them was one inch, and of the other two inches, and found that the ſmaller in three hours loſt 87 grains, and the larger only 77; but in three hours more, the ſmaller further loſt 148,75 grains, and the larger 151. The total loſs of the larger in ſix hours was 228 grains, and of the ſmaller 235,75. But in the month of June he found the larger to loſe in ſeven hours 199 grains, and the ſmaller only 183. That the inequality of temperature in theſe experiments was the true cauſe of the inequality of evaporation, he rendered evident by another experiment, in which, having coated cylinders of unequal capacities with clay, to prevent the quick communication of heat, filled them with water, and expoſed them to the air, he found the

evaporation

evaporation nearly proportional to the ſurfaces, in free contact with the air. 11 Coll. Acad. 142, 143, and 146.

The influence of free contact with the air is farther illuſtrated by the experiments of the Reverend Mr. Williams of Bradford, in New England, for having filled with water two cylinders of equal diameters and depth, and ſupplying the quantity evaporated from one of them once a week, and that evaporated from the other once a month, he found the evaporation from that filled once a week one-third greater than that from the cylinder filled once a month, in the hotter ſeaſon of the year, and ſomewhat below one-third in the colder. 2 American Tranſ. p. 118. The reaſon of which difference was, that the evaporation during a month being greater than that during a week, the air during the laſt three weeks of each month was obliged to ſink deeper into the veſſel than during the firſt week; it was therefore more confined, and could not be ſo quickly renewed, as that over the veſſel that was kept full every week; and therefore during three weeks in each month its evaporation was checked. His veſſels, though in ſome degree expoſed to the air and ſun, were ſheltered from the rain, and therefore in ſome degree from the open air. It ſhould alſo be remarked, that according to the beautiful experiments of Doctor Prieſtly, common water expoſed to the ſun, for ſome time yields oxygen air, which accelerates evaporation while this production

duction laſts, and hence the evaporation from ſuch water is often greater at firſt than afterwards.

The moſt ſtriking proof, and the happieſt application of the efficacy of multiplied ſurfaces in promoting evaporation, may be ſeen in what are called the houſes *of graduation*, where waters, containing only one per cent. of common ſalt, are ſo far evaporated by the mere multiplication of ſurfaces, that the reſidue contains twenty per cent. and in this ſtate, and not before, they are expoſed to heat. Such houſes are common in Germany and Switzerland. See Haller's account of the proceſs in the Paris Memoirs for 1764.

The third circumſtance relative to the ſtate of water, and which affects its evaporation, is its *purity*. It is well known, that concentrated ſolutions of moſt ſalts evaporate much more ſlowly than pure water, in the ſame circumſtances; but Wallerius has ſhewn, that the evaporation from a ſolution of common ſalt, whoſe ſpecific gravity was 1,0466, and conſequently containing ſixteen per cent. was nearly equal to that of ſimple water, after the ſalt was fully diſſolved; but during its ſolution evaporation is diminiſhed by reaſon of the cold produced.

Hence we ſee, that under equal ſurfaces, evaporation from the ſea is as conſiderable as that from lakes.

Wallerius alſo found, that ice evaporates in proportion to its ſurface and temperature, though leſs than water equally cold; and that it evaporates more during congelation than when its congelation is completed. And hence it is, that ice and water evaporate more than mere water of the ſame temperature, and ſo much the more as the air is colder, probably becauſe congelation is promoted by air in proportion as the air is colder; nay, evaporation is checked during a thaw. 11 Collect. Acad. 263, &c.

The evaporation of ice was denied by Baron, but it was aſcertained beyond all poſſibility of doubt by the accurate experiments of Sauſſure. Hygrom § 130. Baron however has proved, that ice expoſed to wind loſes conſiderably by abraſion, the wind carrying off its particles in an exceeding fine powder. Sauſſure alſo obſerved, that ice evaporates in air four or five degrees colder than itſelf.

The *ſtate of the air* in contact with water comes next under our conſideration, as it materially affects the quantity of evaporation. Air may be conſidered either with reſpect to its denſity, or with reſpect to its *temperature*, or with that of its *affinity*, which again involves the conſideration of denſity, or with regard to its *agitation*, or with regard to its *electricity*.

And,

And, 1°. with reſpect to *temperature*, the air in contact with water may be either of the *ſame* temperature as the water, or it may be *warmer*, or *colder*.

If both the water and the air be of the ſame temperature, it appears by the experiments of Richman, that evaporation proceeds moſt ſlowly, as in that caſe the air in contact with the water ſoon becomes ſaturate; and though moiſt air is ſpecifically lighter than dry air, yet that happens only when both are of the ſame temperature, or the dry air colder. Now air in taking up moiſture is cooled by it, and therefore continues its contact with water, and thus the progreſs of evaporation is checked.

Again, if the air be *warmer* than the water, and its temperature between 60° and 70°, and if the water be 14° *colder*, or more, in that caſe there is no evaporation at all, but, on the contrary, the air depoſits the vapours it had previouſly poſſeſſed; for Richman found the ſcale in which the water was at firſt fully counterbalanced, gradually to ſink, and drops of water appeared on the ſides of the veſſel. But if the water be only 11° colder, it will evaporate indeed, but ſlowly. But if the air be at any temperature between 75° and 87°, then evaporation is prevented only when and while the water is 20 degrees colder, or more; and water in Richman's experiments was depoſited on the ſides of

the veſſel, undoubtedly from the air in contact with them. But an inferiority in the temperature of water, amounting to leſs than 20°, will not prevent evaporation, but only proportionably diminiſh it; it is always briſkeſt when the inferiority amounts only to $1\frac{1}{2}$, or 2 degrees.

THEREFORE, the nearer water approaches to the temperature of the air, and yet remains one or two degrees below it, the more evaporation is accelerated. This may help to explain ſome experiments of Achard, 15 Roz. Jour. p. 468.

RICHMAN alſo obſerved that when air is ſome degrees warmer than water, evaporation is for a few minutes ſlower than it becomes afterwards; but after a few hours it is again retarded. The reaſon of which ſeems to be, that the inferiority of temperature in the water is originally greateſt, but gradually diminiſhes, and thus evaporation is accelerated; but, at laſt, the water attains an equality of temperature, and then evaporation is retarded.

THIS, however, can rarely happen, but when the maſs of water is ſmall, or its depth inconſiderable, or the air in a ſtagnating ſtate.

2°. WHEN *air is colder* than the water expoſed to it, the reſult, with reſpect to evaporation, is exactly the reverſe of what happens when

when the water is the colder of the two. For Richman's experiments ſhew, that other circumſtances being ſimilar, evaporation in equal times (at leaſt in portions of time not exceeding ſeven hours) is ſo much the greater, as the difference between the temperature of the air and that of the water is greater. Or, in other words, the *evaporation is as the differences between the colder air and the warmer water.* Thus, if a difference of 92° produces an evaporation as 13,5, a difference of 68° will produce an evaporation of 9,46. This law, however, reſts on the ſuppoſition, that the temperature remains unaltered (otherwiſe a new calculation would be requiſite), which may be ſuppoſed nearly true, when large maſſes of water in contact with the air are cooled by it; they deſcend and give place to the lower and warmer water, while the air, warmed by contact with the water, riſes, and gives place to the colder which deſcends: but ſuch retrogade movements, whether of the water or of the air, are imperceptible, unleſs the differences of temperature be conſiderable in the different ſtrata of each; and with reſpect to air in particular, unleſs the ſpecific gravity of the lower ſtrata be inferior to that of the upper in a notable degree. The previous ſaturability of air, (that is, its diſtance from the point of ſaturation) muſt occaſion conſiderable differences in the evaporation of the water expoſed to it.

Lastly, if we ſuppoſe the temperature of the water conſtant, and the temperature of the air in one caſe ſome degrees hotter, and

and in another caſe colder than water, by an equal number of degrees, it ſhould ſeem, that in equal times the evaporation would be greater in the hotter air than in the colder, becauſe colder air is ſooner ſaturated, requiring only to produce ſaturation a ſmaller proportion of moiſture; yet as, on the other hand, hot air depoſits moiſture on water ſome degrees colder, this point is not yet abſolutely ſettled.

Section II.

Of the Influence of Affinity.

The next property of air materially concerned in promoting evaporation is its *affinity* to water. That this affinity is very ſtrong, appears by the difficulty of depriving water of the air it contains, either by ebullition, or by the action of the air-pump, as Mr. De Luc has long ſince ſhewn. This affinity, nevertheleſs, does not ſeem to me a chemical affinity, for water and air do not form a *tertium quid*, as bodies chemically united do; and they are in great meaſure ſeparable by mere mechanical means, as by the action of the air-pump, which bodies chemically united never are, and hence, Sauſſure very properly calls it, an hygrometrical affinity: yet it reſembles chemical affinity thus far, that it acts by attraction, and promotes an union, at leaſt of adheſion, and this attraction has its limit, which is alſo called *ſaturation*.

Of

Of ſaturation, or of any approach towards it, no ſatisfactory knowledge could be obtained previous to the diſcovery of a comparable hygrometer by Mr. Sauſſure; an invention which perhaps is equal in importance, and conſidering it conjointly with the inveſtigation of the numerous incidents to which it gave birth, was attended with greater difficulties, and required more ſagacity, than any diſcovery, (thoſe of Scheele excepted) made during the courſe of the laſt century.

This inſtrument conſiſts of a ſingle human hair, duly cleanſed, and kept in a ſtate of tenſion by the weight of a few grains, and rolled on a pulley to which an index is fixed. The hair is lengthened by moiſture, and the index drawn up; it is contracted by dryneſs, and the index drawn down; the weight prevailing in the firſt caſe, and the elaſticity of the hair in the ſecond caſe: heat alſo lengthens the hair, but as it alſo promotes the evaporation of the moiſture adhering to the hair, it by that means, upon the whole produces a ſtill greater contraction, and thus makes it indicate ſuperior dryneſs.

Fixed points both of the *maximum* of deſiccation, and of the *maximum* of moiſture, that is ſaturation therewith, were accurately ſettled, and even ſome degrees of ſuperſaturation noted. The *maximum* of deſiccation is not undoubtedly the *true maximum*; yet

yet however, ſince it far exceeds any variable degree of moiſture, ever diſcovered in air, it is fully ſufficient for all meteorological purpoſes.

THE *maximum* of deſiccation is marked o on the ſcale of this inſtrument*, and the *maximum* of moiſture, or point of ſaturation, 98°. To prevent confounding theſe with thermometrical degrees, which often accompany them, I denote them by Roman numerals. XXV. marks the greateſt dryneſs, which according to Sauſſure can never be found in the open air†. XL. the greateſt dryneſs ever obſerved by him; conſequently even L denotes a conſiderable degree of deſiccation. But LXX denotes a degree of moiſture often perceptible on glaſs.

HERE it muſt be carefully remarked, that hygrometrical degrees denote very nearly the *ratios* which the quantity of moiſture poſſeſſed by a *ſtratum* of air, bears to the quantity that would ſaturate that air in its *actual* temperature; and as this ſaturating quantity varies more or leſs in every degree of temperature, it follows,

* I have often produced a deſiccation far below o by means of ſulphuric acid highly concentrated.

† I dont know that the dryneſs of the ſcirocco or harmattan was ever explored by this inſtrument.

follows: 1°, that the *ſame ratio*, or hygrometrical degree, will indicate a different ſaturating quantity, and a different abſolute quantity of moiſture, in every different degree of temperature.

2d°. That if in a given *ſtratum* of air the temperature be altered, while the quantity of moiſture remains conſtant, the ratio or degree will alſo be altered; becauſe the quantity neceſſary to ſaturation is altered: therefore, if in this caſe the temperature be raiſed, the index will be lowered, and mark a greater dryneſs, becauſe the air is farther from ſaturation, and therefore relatively dryer; and, *vice verſa*, if the temperature be lowered, the index will aſcend, and mark a greater degree of moiſture, though its real quantity be the ſame, becauſe the air is then nearer to the point of ſaturation than it was before.

This ratio of the actual degree to the ſaturating quantity is what I call the relative *ſaturability* of air.

Hence it follows that, at high temperatures, the ſame hygrometrical degree denotes an abſolute quantity of moiſture far greater than it does at low temperatures; becauſe, though the ratios to the ſaturating quantity are nearly the ſame, yet the ſaturating quantity in high temperatures is far greater than in low temperatures, and a high hygrometrical degree in air at a *low* tempe-

rature may indicate a far ſmaller quantity of moiſture than a low hygrometrical degree in a *high* temperature.

Saussure did not ſtop at theſe general indications. He has given us, 1° The *abſolute* quantities of moiſture, correſponding with each hygrometrical degree, contained in the French cubic foot (that is 1,210 Engliſh) at the temperature of 15°,16 of Reaumur, equal to 65,8 Fahr. or rather 65,4, for he neglects the fraction 0,16: 2do. the abſolute quantities of moiſture in the cubic foot, correſponding with every five degrees of the hygrometer, commencing at the XLth, at every fifth degree of Reaumur, commencing at — 10 = 9°,5 of Fahr: but on the accuracy of this table he does not ſo much rely, as on that of the firſt. Fortunately, extreme accuracy for the general purpoſes of meteorology is not requiſite: 3tio. he has given a table of the diminution of the menſtrual power of air when rarefied to the degrees denoted by barometrical heights. With reſpect to the principles on which this table is founded I do not however perfectly agree with him.

Of the firſt table I ſhall here tranſcribe ſuch degrees as appear to me moſt curious and uſeful.

TABLE

TABLE I.

Of the Weight of Moiſture in a Cubic Foot, indicated by the Hygrometrical Degrees at 65° of Fahr. Barometer 28,77. (Engliſh).

Hygrometer.	Grains.	Hygrometer.	Grains.	Hygrometer.	Grains.	Hygrometer	Grains.
I	0,0304	L	3,4852	LXVII	5,8595	LXXXIV	8,7170
II	0,0643	LI	3,5902	LXVIII	6,0329	LXXXV	8,8850
XXXV	2,1702	LII	3,6976	LXIX	6,1971	LXXXVI	9,0530
XXXVI	2,2475	LIII	3,8072	LXX	6,3651	LXXXVII	9,2210
XXXVII	2,3254	LIV	3,9192	LXXI	6,5331	LXXXVIII	9,3890
XXXVIII	2,4041	LV	4,0335	LXXII	6,7011	LXXXIX	9,5570
XXXIX	2,4834	LVI	4,1502	LXXIII	6,8691	XC	9,7250
XL	2,5634	LVII	4,2692	LXXIV	7,0370	XCI	9,8930
XLI	2,6451	LVIII	4,3905	LXXV	7,2050	XCII	10,0610
XLII	2,7291	LIX	4,5141	LXXVI	7,3720	XCIII	10,2290
XLIII	2,8155	LX	4,6534	LXXVII	7,5410	XCIV	10,3970
XLIV	2,9042	LXI	4,8021	LXXVIII	7,7090	XCV	10,5650
XLV	2,9952	LXII	4,9597	LXXIX	7,8770	XCVI	10,7330
XLVI	3,0885	LXIII	5,1271	LXXX	8,0450	XCVII	10,9010
XLVII	3,1842	LXIV	5,3031	LXXXI	8,2130	*XCVIII	11,0690ſatⁿ.
XLVIII	3,2822	LXV	5,4873	LXXXII	8,3810	XCIX	ſuperſatⁿ.
XLIX	3,3826	LXVI	5,6775	LXXXIII	8,5490		

*He originally fixed ſaturation at C; but as ſome uncertainty attended this point, he afterwards thought it ſafer to fix it at XCVIII.

As air at each degree of heat may contain various degrees of moiſture, from the extreme degree of dryneſs up to the extreme or ſaturating degree of moiſture, *every* hygrometrical degree may be found in *each* degree of temperature, and conſequently tables ſimilar to the laſt might be formed for each degree of temperature. To form ſuch tables with due accuracy would require the labour of many years. Hence Sauſſure, in the ſecond table, has only marked the quantity of moiſture in a cubic foot, correſponding with a few of the hygrometrical degrees at each 5th degree of Reaumur, from —10° to + 30° Reaumur.

The ſaturating quantity of moiſture in a cubic foot I call its hygrometrical *complement*. As Sauſſure has given the complements of only every fifth degree of Reaumur, I have interpolated four arithmetical means for the complements of the intermediate degrees, but to diſtinguiſh them from thoſe of Sauſſure, I have added S to theſe laſt.

I have alſo given Fahrenheit's degrees under thoſe of Reaumur.

This table I have contrived to fill by taking the ratios, each hygrometrical quantity, at each degree of heat, bears to the complement, as ſhewn in page 310, but I have calculated only to 80° of Fahr.

TABLE

T

Of the Quantities of Moiſture in a cubic Foot

Reaumur	10°	9°	8°	7°	6°	5°	4°	3°	2°	1°	0°	1°	2°
Fahren.	9°,5.	11°,75	14°	15°,12	18°,5	20°,75	23°	25°,25	27°,5	29°,75	32°.	34°,25	36°,5
Hygrometer	S					S					S		
XL	0,8971	,9371	,9789	1,0206	1,0625	1,1067	1,1595	1,2066	1,2593	1,3101	1,3653	1,4260	1,4910
XLV	1,0676	1,1509	1,1653	1,2151	1,2890	1,3171	1,3760	1,4375	1,4987	1,5589	1,6248	1,6710	1,7778
L	1,2197	1,289	1,2954	1,3871	1,4442	1,5047	1,5712	1,5915	1,7112	1,7800	1,8563	1,9378	2,0251
LV	1,4116	1,476	1,5427	1,6085	1,6742	1,7414	1,8218	1,9029	1,9844	2,0635	2,1483	2,2470	2,3420
LX	1,6411	1,7119	1,7882	1,8647	1,9412	2,0246	2,1116	2,2060	2,3004	2,3923	2,4976	2,6050	2,7210
LXV	1,9204	2,008	2,0975	2,1868	2,2767	2,3691	2,4769	2,5878	2,6980	2,8061	2,9226	3,0550	3,1910
LXX	2,2277	2,338	2,4336	2,5463	2,6437	2,7482	2,8768	3,0054	3,1341	3,2592	3,3903	3,5490	3,7071
LXXV	2,5215	2,637	2,7542	2,8720	2,9896	3,1107	3,2525	3,3973	3,5425	3,6847	3,8375	4,0125	4,1908
LXXX	2,8155	2,944	3,0770	3,2071	3,3390	3,4734	3,6325	3,7943	3,9574	4,1153	4,2850	4,4803	4,6800
LXXXV	3,1095	3,253	3,3980	3,5425	3,6886	3,8361	4,0124	4,1919	4,3702	4,5457	4,7324	4,9489	5,1699
XC	3,4035	3,562	3,7197	3,8708	4,0379	4,1987	4,3924	4,5889	4,7852	4,9763	5,1797	5,4176	5,6598
XCV	3,6946	3,861	4,0325	4,2025	4,3781	4,5578	4,622	4,9751	5,1884	5,3977	5,6227	5,8751	6,1370
XCVIII	3,8739	4,0549	4,2359	4,4169	4,5979	4,7790	5,0023	5,2256	5,4489	5,6722	5,8956	6,1701	6,4446

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TABLE II.

Of the Quantities of Moisture in a cubic Foot of Air at different Degrees of the Hygrometer and Reaumur's Thermometer.

Reaumur	10°	9	8°	7°	6°	5	4°	3°	2°	1°	0°	1°	2°	3°	4°	5°	6°	7°	8°	9°	10°	11°	12°	13°	14°	15°	16°	17°	18°	19°	20°	21°
Fahren.	57°,5	59°,75	64°	65°,12	68°,25	70°,75	73	75°,25	77°,5	79°,75	82°.	84°,25	86°,5	88°,75	91°,	93°,25	95°,5	97°,75	100	102°,25	104°,5	106°,75	109°	111°,25	113°,5	115°,75	118°	120°,25	122°,5	124°,75	127°	129°,25
Hygrometer	S					S				S						S					S					S					S	
XL	0,8571	0,9371	0,9789	1,0206	1,0625	1,1067	1,1595	1,2066	1,2593	1,3101	1,3653	1,4266	1,4910	1,5532	1,6304	1,6843	1,7595	1,8379	1,9122	1,9927	2,0779	2,1710	2,2675	2,3649	2,4554	2,5634	2,6721	2,7973	2,9144	3,0370	3,1625	3,3033
XLV	0,9204	1,0009	1,1509	1,1653	1,2151	1,2890	1,3171	1,3760	1,4375	1,4987	1,5581	1,6248	1,6710	1,7775	1,8485	1,4514	2,0045	2,0951	2,1878	2,2813	2,3749	2,4729	2,583	2,6991	2,8140	2,9496	2,9952	3,1241	3,3298	3,4741	3,6151	3,6952	3,9320
L	1,0197	1,089	1,1954	1,3871	1,4412	1,5047	1,5712	1,5915	1,7112	1,7800	1,8563	1,9378	2,0251	2,1112	2,2636	2,2900	2,3913	2,2782	2,6047	2,7115	2,8251	2,9575	3,0120	3,2230	3,3372	3,4852	3,7262	3,8[illegible]	3,9645	4,1276	4,2997	4,4892
LV	1,1116	1,176	1,2427	1,3085	1,6742	1,7414	1,8218	1,9029	1,9844	2,0635	2,1483	2,2370	2,3420	2,4462	2,5836	2,6503	2,7723	2,8962	3,0203	3,1442	3,2696	3,4206	3,5753	3,7260	3,8694	4,0335	4,3212	4,4082	4,5966	4,7899	4,9761	5,2055
LX	1,2411	1,7119	1,7884	1,8647	1,9412	2,0246	2,1116	2,2060	2,3004	2,3923	2,4976	2,6050	2,7210	2,8368	2,996	3,0660	3,2145	3,358	3,5011	3,6437	3,7737	3,8523	4,1422	4,3102	4,4856	4,6554	5,[illegible]	5,1103	5,3256	5,5481	5,7434	6,0345
LXV	1,9204	2,008	2,0975	2,1868	2,2767	2,3691	2,4769	2,5878	2,6980	2,8061	2,9226	3,0550	3,1910	3,3280	3,5210	3,6055	3,7624	3,9332	4,1064	4,2747	4,4480	4,6510	4,8584	5,0659	5,2612	5,457[illegible]	5,[illegible]	5,9948	6,2499	6,5073	6,7697	7,0779
LXX	2,277	2,338	2,4336	2,5463	2,6437	2,7482	2,8768	3,0251	3,1341	3,2592	3,3903	3,5490	3,7071	3,8659	4,0840	4,1624	4,3792	4,5745	4,7700	4,9652	5,1596	5,4023	5,[illegible]	5,[illegible]	6,1112	6,3651	6,[illegible]	7,250	7,4563	7,[illegible]	7,8726	8,7316
LXXV	2,5115	2,637	2,7542	2,8720	2,9896	3,1107	3,2525	3,3973	3,5425	3,6847	3,8375	4,0125	4,1900	4,3688	4,6131	4,7242	4,[illegible]	5,1712	5,3920	5,6129	5,8404	6,1067	6,4679	6,6516	6,9080	7,2050	7,7146	7,8700	8,2063	8,5442	8,8888	9,2935
LXXX	2,7155	2,944	3,0770	3,2071	3,3390	3,4734	3,6325	3,7943	3,9574	4,1153	4,2850	4,4863	4,6802	4,8789	5,1543	5,2872	5,5232	5,7753	6,0220	6,2689	6,5213	6,8210	7,1249	7,4290	7,7155	8,0450	8,6163	8,7895	9,1652	9,5427	9,9251	10,380
LXXXV	3,0155	3,253	3,3980	3,5425	3,6886	3,8361	4,0124	4,1919	4,3702	4,5457	4,7324	4,9189	5,1699	5,[illegible]	5,6904	5,[illegible]	6,1271	6,3764	6,6523	6,9250	7,2022	7,5332	7,8709	8,2072	8,5230	8,8850	9,5179	9,7100	10,125	10,543	10,[illegible]	11,466
XC	3,3[illegible]	3,562	3,7197	3,8[illegible]	4,0379	4,1987	4,3924	4,5[illegible]	4,7552	4,9763	5,1797	5,4176	5,6[illegible]	5,[illegible]	6,2295	6,3[illegible]	6,5325	6,[illegible]	7,2885	7,5872	7,8831	8,2550	8,6205	8,9840	9,3304	9,7250	1[illegible]	10,630	11,084	11,540	11,9977	12,552
XCV	3,6[illegible]	3,861	4,0[illegible]	4,2025	4,3781	4,5578	4,622	4,9751	5,1884	5,3977	5,6227	5,8751	6,1370	6,3[illegible]	6,7545	6,9420	7,2491	7,[illegible]	7,[illegible]	8,[illegible]	8,[illegible]	8,9429	9,3421	9,7410	10,1153	10,5650	11,[illegible]	11,525	12,045	12,512	13,0340	13,609
XCVIII	3,9[illegible]	4,0549	4,2359	4,4169	4,5979	4,7790	5,0023	5,2256	5,4489	5,[illegible]	5,[illegible]	6,1701	6,4446	6,7191	7,0936	7,2731	7,6129	7,[illegible]	8,2925	8,6323	8,[illegible]	9,3918	9,8111	10,2304	10,6497	11,0690	11,5865	12,1040	12,6215	13,1390	13,6558	14,2940

As the lower quantities of moiſture, correſponding to the lower hygrometrical degrees at each degree of heat, bear to the *complement* of each degree of heat, reſpectively, nearly the ſame ratio, the blanks in the foregoing table were filled up with tolerable accuracy, (until the quantities are determined by experiment) by means of the firſt table for temperature 65° Fahr. in which both the complement and the inferior quantities were accurately determined, and which I therefore call the *ſtandard table*.

For, *as this ſtandard complement for* 65°, *is to any inferior quantity of moiſture, at any hygrometrical degree in that table; ſo is the complement for any other degree of heat to the inferior quantity of moiſture appertaining to the ſame hygrometrical degree, at that given degree of heat*, at leaſt ſufficiently near.

Stand. Compl. (at L.) : Compl. 32° (at L.)

Thus as 11,069 : 3,4852 : : 5,8956 : 1,842. Sauſſure found this laſt 1,8563. But in higher degrees of heat the approximation is greater.

Stand. Compl. (at XCV.) Compl. 99° (at XCV.)

Thus as 11,069 : 10,565 : : 20,784 : 19,8376, and Sauſſure found it 19,8379.

To

To form this table from actual experiment from the XL. of the hygrometer to XCVIII. and from the 9th of Fahr. to 99°, would require 5220 experiments, of the niceſt and moſt delicate kind.

Saussure has given various other uſeful tables, for which I muſt refer to his treatiſe; but he has given one, to the principle of which I cannot entirely aſſent. He is of opinion, that the menſtrual power of air diminiſhes with its denſity, § 146; and hence infers, that its power of *receiving* vapour is alſo diminiſhed when it is rarefied. This laſt point, however, is contradicted by the quick evaporation that takes place in *vacuo* and on lofty mountains.

His own experiments ſeem to me to prove this increaſe of the receptive power of air when its denſity was diminiſhed; for he conſtantly found his hygrometer to move to dryneſs during every ſucceſſive exhauſtion, and permanently ſo, when the acceſs of new vapour was prevented, § 145. Hence it appears, that the greateſt part of the moiſture, originally contained in his hygrometer, being no longer confined by the denſity of the ambient air, gradually evaporated.

Mr. Pictet's experiments on the manner in which hygrometers are affected by mere vapour excite much ſurprize, and deſerve much attention. Eſſais de Phyſ. § 111.

He

He filled with vapour a receiver previouſly exhauſted of air; the receiver contained an hygrometer which ſtood at XCVIII, and conſequently marked ſaturation with moiſture; and the temperature, both under the receiver and in the room, was 41°; no moiſture was depoſited on the glaſs. The receiver, in this ſtate, was carried into another room, whoſe temperature was 32°; here moiſture, and even drops of water, immediately appeared on the glaſs, and the hygrometer indicated greater dryneſs, for it ſtood at XCI. And ſo I think it ſhould, for the quantity and denſity of what vapour remained in the air were really diminiſhed, drops having been formed from part of it; but after twenty minutes, the hygrometer again moved to moiſture, and finally ſtood at XCVII and one-half, and the cold under the receiver was increaſed ſix degrees. This too, it appears to me, ſhould naturally be expected; for, by the increaſe of cold, not only the vapours in contact with the hair are more condenſed, but its affinity to moiſture is alſo increaſed; it therefore abſorbs more of them, and conſequently moves to moiſture. On moving the receiver, in this laſt ſtate, into a room whoſe temperature was 45°,5, the drops condenſed on its interior began to evaporate, and while this evaporation laſted, the hygrometer indicated an increaſe of moiſture, but ſoon afterwards it moved to dryneſs, and in three hours the temperature, being then nearly 48°, it ſtood at XC, becauſe the heat being 16° higher, diminiſhed the affinity of the hair to moiſture,

and

and cauſed part of it to ſeparate from its interior; at leaſt I ſee no other cauſe.

Although the Engliſh cubic foot be ſmaller than the French, and the French grain ſmaller than the Engliſh, yet I have left the quantities given by Sauſſure unaltered, becauſe I am perſuaded that even the Engliſh cubic foot of air, at leaſt in temperatures exceeding 60°. contains at every hygrometrical degree much more moiſture than is indicated by theſe tables. This our author acknowledges, § 47, and well explains why the whole of the moiſture contained in a cubic foot of air can never be extracted; beſides it ſeems to me, that under the veſſels in which his experiments were made, the air not having ſufficient power to expand itſelf as it ſhould, on receiving moiſture, could not take up the whole quantity which, were it at liberty, it would take up in high temperatures, that is above 75°, and therefore appeared ſaturated, when it, if at liberty, would not be ſo.

As both ſaturability in a given heat, and alſo an increaſe of heat, promote evaporation, it may be aſked, which of them exerts the greateſt influence? This curious queſtion Sauſſure has beautifully reſolved from obſervations on evaporation on the *Col de Geant*, compared with thoſe made at *Geneva*, and by an ingenious calculation of the reſulting effect of each of them. Voy. aux. Alpes, § 2060.

The

The ſaturability of air he eſtimates by the *number of grains*, over and above thoſe it already poſſeſſes, which it requires to arrive at ſaturation, in its actual temperature. Theſe grains he therefore calls *degrees* of ſaturability (or dryneſs). He thus found: 1°. that in air, whoſe denſity is about one-third ſmaller than uſual (as where the barometer ſtands at twenty inches) the influence or evaporating effect of a difference of one degree of Reaumur (2°,25 Fahr.) is ſomewhat more than three times greater than the influence of a difference of one degree of dryneſs or ſaturability.

2do. That in air of the uſual denſity, or only a few inches above or below it, the influence of a difference of one degree of ſaturability is about three times greater than the influence of a difference of one degree of Reaumur (2,25 Fahrent.) Therefore in an atmoſphere of the uſual denſity, on plains, the influence of a difference of nearly 7° of Fahrent. is nearly equal to the influence of a difference of one degree of ſaturability.

Hence our northerly and eaſterly winds, though ſome degrees colder than the ſoutherly and weſterly, yet promote evaporation much more; the air they convey being farther from ſaturation. Thus Achard found the evaporation from his atmedometer to be 13, on the 4th of May 1788, under an ENE. wind, thermometer 51°, hygrometer XLVIII. but on the 7th of the ſame month the evaporation was but 8, the wind being WSW. though the

mean heat of the day was nearly 60°, the hygrometer being at LVIII. and consequently nearer saturation. Mem. Berl. 1788, p. 126.

To give some idea of the saturability of air at each 5th degree of the hygrometer from XL. to XCV. at any temperature of the air from 9°,5 to 99°5, I have calculated the following table, which exhibits very nearly the ratio of the hygrometrical degrees to the complement or saturating quantity (at each degree of heat) taken as 1,000.

XL . . 0,2311. Thus the complement of 9°,5 is 3,8739, of which 0,2311 is between $\frac{1}{5}$ and $\frac{1}{4}$.

XLV . . 0,275
L . . 0,314
LV . . 0.364
LX . . 0,422
LXV . . 0,495
LXX . . 0,575
LXXV . . 0,650
LXXX . . 0,726
LXXXIII . . 0,80
LXXXV . . 0,802
XC . . 0,878
XCV . . 0,952

Thus

Thus Sauſſure found the complement of 9,5 to be 3,8739 grains, and the quantity at XL. to be 0,8971 of a grain, and by the ratio here given it is 0,8952 of the complement.*

Section III.

Of the Influence of Wind.

The third agent concerned in producing evaporation is agitation; whether of water in quieſcent air, or of air itſelf, that is *wind*, which neceſſarily produces the agitation of the water expoſed to it. It is evident that the agitation of water promotes evaporation by augmenting its ſurface to a degree that eſcapes calculation. The agitation of air, beſides involving that effect, promotes evaporation by the rapid removal of vapours as faſt as they are formed, and by inceſſantly preſenting new ſurfaces of leſs ſaturated air, and cooling the water if it be warmer than the air, in which caſe we have already noted the evaporation to be conſiderable, or, if both be of the ſame temperature, by lowering that of the water, or if the air be conſiderably warmer than the water, by lowering the difference, and conſequently promoting evaporation, as already ſhewn. Moſt of theſe effects of wind are proportioned to its ſaturability, of which enough has been already ſaid, and to its velocity, of which I ſhall now treat.

* How obſervations ſhould be made on this inſtrument may be ſeen in the Appendix, Sauſſure, 309.

Mr. Smeaton has communicated an excellent table of the velocity of wind, which I here tranſcribe from the Phil. Tranſ. 1759, p. 165.

Miles per Hour.	Feet per Second.	Perpendicular Force on one ſquare Foot, in Avoirdup. Pounds and Parts.	
1	1,47	.,005	Hardly perceptible.
2	2,93	.,020	Juſt perceptible.
3	4,4	.,044	
4	5,87	.,079	Gently pleaſant.
5	7,33	.,123	
10	14,67	.,492	Pleaſant, briſk.
15	22,	1,107	
20	29,34	1,968	Very briſk.
25	36,67	3,075	
30	44,01	4,429	High wind.
35	51,34	6,027	
40	58,68	7,873	Very high wind.
45	66,01	9,963	
50	73,35	12,300	Storm or tempeſt.
60	88,02	17,715	Great ſtorm.
80	117,36	31,490	Hurricane.
100	146,7	49,200	Hurricane that tears up trees and carries buildings before it.

And Condamine tells us, that eighty-five feet per ſecond is the moſt violent ſtorm whoſe velocity can be meaſured at ſea. Mem. Paris, 1757, p. 637, 8vo.

Derham and Kraft's obſervations agree with thoſe of Smeaton. By means of this table*, if the velocity of wind be known by any inſtrument, its force may alſo be diſcovered, and *vice verſa*. The effect of the velocity of wind, inaſmuch as it reſpects evaporation, Sauſſure has detected, by new and ingeniouſly contrived, though indirect experiments, on *Col de Geant* and at *Geneva*, and found that air, moving at the rate of forty feet per ſecond, triples the quantity of evaporation that would happen in calm air. He has ſhewn, that in this caſe alſo, the greater or leſſer ſaturability of the air is a circumſtance that materially affects the magnitude of the effect. That on plains, a difference of one degree of ſaturability is equivalent to a difference of 11° Fahr. of heat; and on heights, on which the barometrical height is diminiſhed one-third, its influence is equal to that of heat.

Hence we ſee, that the warmer air is, the quicker its motion, the greater its ſaturability, and the longer its duration, the more it will promote evaporation.

Section

*The beſt portable inſtrument for meaſuring the force of wind is that invented by Dr. Lind, Phil. Tranſ. 1775.

Section IV.

Of the Influence of Electricity and Light.

That *electricity* powerfully aſſiſts evaporation is generally acknowledged. Sauſſure has ſhewn that a card, ſuperſatured with moiſture, loſt, when electrified, two grains in a quarter of an hour, while another equally circumſtanced, but not electrified, loſt only $1\frac{1}{2}$ grain: but he limits its action to water in a free and uncombined ſtate. Hygrometer: § 159 and 160. Briſſon alſo has obſerved, that an excited electric raiſes water above its level. Mem. Par. 1767, p. 328, 8vo. But how it acts in the operations of nature is but imperfectly known.

Light alſo contributes to evaporation by diſengaging air from water, and perhaps for other reaſons as yet imperfectly known.

Section V.

Of the joint Action of the above-mentioned Cauſes.

The only ſeries of obſervations with which I am acquainted, made on evaporation, in which the principal cauſes that concur in producing it admit of a diſtinct conſideration, being marked by accurate inſtruments and preciſe meaſures, are thoſe of Mr. Achard

Achard, to be found in the Berlin Memoirs for the years 1788 and 1790. Of theſe I ſhall tranſcribe a few, accompanying them with remarks, omitting the barometrical heights, as their differences, conſiſting only of two or three tenths of an inch, could not materially affect the quantity of evaporation.

1788	Mean Temperature.	Mean Hygrom. Degree.	Evaporation Ratio.	Wind.	Force Mean.
May 1ſt	61,25	LII.	,001	NE.	5.3.
17th	56,	LIV.	,004	NE.	5.5
July 5th	72,5	L.	,014	SW.	5.5
1789					
July 2d	54,5	XLIX.	,001	W.	25.10

The force of the wind is marked twice, namely at each period of obſervation, that is at ſeven o'clock in the morning, and at twelve o'clock. That which obtained during the remainder of the day is not noted, which renders theſe obſervations incapable of exact calculation, the degrees of the other inſtruments and of evaporation

evaporation were taken three times in twenty-four hours, namely at ſeven and twelve o'clock in the day, and at ten at night.

On theſe obſervations I remark :

1o. That in the firſt and fourth, the quantities evaporated were equal, though the temperatures of the air differed conſiderably, viz. 6°,25, that, during the firſt, being ſo much warmer than during the laſt. This equality can be attributed only to the greater ſaturability of the air in the laſt caſe, and the greater force of the wind.

2do. That in the ſecond obſervation, the evaporation was four times greater than in the firſt, though the temperature was 5° lower and the hygrometer 3 degrees higher, yet the ſaturabilities at the reſpective temperatures did not differ much, as may be ſeen by the foregoing table, and the force of the wind was much greater during the ſecond obſervation.

3tio. That in the third obſervation, the evaporation was by far the greateſt, though the force and duration of the wind were (at leaſt to appearance) exactly the ſame as in the ſecond obſervation, becauſe the heat was 16° higher, and the hygrometer 4 degrees lower in the ſecond obſervation.

Thus

Thus we ſee how obſervations duly made might be analyſed, and the general reſult calculated *a priori*.

It deſerves alſo to be remarked, that though Sauſſure never found the air in Switizerland drier than XL and frequently at XCV. or even ſaturated, yet Achard, at Berlin, during the years 1788 and 1799, never found it moiſter than LXX. generally between L. and LX. and frequently at XL. and once even at XXXII. Berlin ſtands on a ſandy ſoil. The mean height of the barometer is 29,84 inches, and conſequently its height over the level of the ſea is about one hundred and forty feet, its diſtance from the Baltic one hundred and twenty miles, whereas Geneva ſtands on the border of a great lake. The mean height of the barometer is 28,77 inches, and conſequently one thouſand and ninety-one feet above the level of the ſea, (others deem it one thouſand two hundred and twenty) and about two hundred and twenty-five miles diſtant from the Mediterranean. The air in mountainous countries is known to be much moiſter than that over flat countries.

During the ſeven laſt months of the year 1785, and the ſeven firſt months of 1786, I made daily obſervations, and never found the hygrometer to ſtand lower than LXV. and that was on the 22d of April 1785, barometer 29,97, thermometer 56°, wind

NW. moderately ſtrong. The uſual ſtate of the hygrometer was from LXXV. to XC. and often ſtill higher.

The greateſt loſs by evaporation which I ever obſerved in one hour, from my atmedometer, was forty-five grains, its ſurface was 25,23 ſquare inches, and it contained 23,13 cubic inches of water, barometer 30,00 inches, thermometer (which I always keep ſhaded from the ſun) 68°,5, hygrometer LXXIX. initial heat of the water 66°, final 64°, wind E. moderately ſtrong; and the ſmalleſt loſs in one hour was two grains, but frequently I could perceive no loſs during the firſt hour, and ſometimes ſome loſs during the firſt, ſcarce any during the ſecond, the water being frozen.

Thus on the 1ſt of March 1785, barometer 30,15, thermometer from 23° to 25°, wind N. initial heat of the water 32°, final 32°, but frozen hard, it loſt five grains the firſt hour and one on the ſecond hour, while frozen. The quantity of water in a cubic foot of air I found by ſulphuric acid to be 5,4 grains: I had then no hygrometer, but by Sauſſure's table it could not be far from ſaturation. But I have frequently obſerved a loſs of from ſix to eight grains during the firſt hour, thermometer 26°, and even ſix grains, when frozen, when the atmedometer was fully expoſed to the open air. But during foggy and calm days there

there was often no evaporation. Including every variable circumſtance, I conclude the hourly evaporation at 32° may be eſtimated at four grains, and at 45° at ten grains, at 50° at twelve grains, at 55° at ſixteen grains, at 65° at twenty grains, and at 75° at twenty-ſix grains, at a mean throughout the year, in countries where the mean hygrometrical height is LXXXV. and ſtorms not very frequent. Hence we may calculate the annual evaporation from maſſes of water in countries whoſe mean annual temperature and hygrometrical ſtate and agitation by tempeſts is known. This circumſtance, however, has not hitherto been ſufficiently attended to, nor conſequently even approximately determined.

In London, whoſe mean annual temperature is about 50°, and where the hygrometer, at a mean of the year, is probably LXXXV. (I ſay probably, becauſe its ſtate in dry and moiſt years has not been determined) ſince the *hourly* evaporation may be ſtated at twelve grains, the *daily* may be ſtated at a mean of the year at two hundred and eighty-eight from a ſurface of twenty-ſix ſquare inches, and therefore from one ſquare foot one thouſand five hundred and ninety-five grains, which multiplied into three hundred and ſixty-five gives 582,175 grains = 83,1676 pounds avoirdupoiſe, nearly, or about 101,07 pounds troy. Such a quantity of water, in a veſſel of one ſquare foot in the bottom and

aperture, would riſe to the height of upwards of 15,76 altitudinal inches, which is nearly the quantity of rain that falls in London in the drieſt years.

Doctor Hales, from ſome experiments, concludes the evaporation from a ſurface of earth to be nearly the ſame in winter as in ſummer, as it contains more moiſture in the former ſeaſon, and ſtates its proportion to that from water as three to ten, but by *earth* he means earth diveſted of vegetables, which ſcarce ever occurs, 1 Hales, p. 55; and with his experiments thoſe of the Reverend Mr. Williams, of Bradford, in New England, nearly agree, 2 Amer. Tranſ. 120. But the evaporation from a ſquare foot of earth covered with vegetables, and duly ſupplied with moiſture, is much greater than that from mere water, as follows from his experiments, and ſtill more demonſtrably from the great and ſpeedy evaporation from the multiplied ſurfaces in houſes of graduation. In a cultivated country, like the vicinity of London, it is to that from water at leaſt as fifteen to ten. Bazin, from two years obſervations on moiſt earth, infers that it evaporates much more quickly than an equal volume of water. Hiſt. Par. Acad. 1741.

Rain does not prevent evaporation, as I have frequently obſerved.

Saussure's

SAUSSURE's experiments on evaporation in tranquil air, both on Col de Geant, where the barometer ſtood at 20 inches, and at Geneva, where it ſtood at 28,77, bear ſome analogy to theſe, as they were not made on mere water, but on a piece of linen about thirteen inches long and ten broad, moiſtened until it weighed one hundred and fifty grains more than when dry, then expoſed to evaporation for twenty minutes and weighed.

On the Mountain.

Thermr.	Hygromr.	Saturability.	Evaporation.
51°	LXXIV	4,032	39,5 grs.
42°,5	XC	1,324	20,8
43°,5	LXXXV	2,184	24
			Mean 29,7
	On the Plain.		
54,5	LXXXIII	2,495	19,75
48,76	LXXXIII	2,384	14,5
46,25	LXXXI	2,772	13,75
			Mean 16,00

HERE

HERE two ſurfaces were expoſed to evaporation, each conſiſting of one hundred and thirty ſquare inches. The time of expoſition was one-third of that of my experiments, but the ſurfaces were ten times greater. The mean temperature of the three experiments on the plain was 49°,5, the mean hygrometrical height LXXXII. and the mean quantity evaporated ſixteen grains. This reſult differs conſiderably from the mean of mine made at nearly the ſame temperature, but different degrees of moiſture in the air, and under a ſurface ten times ſmaller, and in a portion of time three times greater. If his ſurfaces and times were reduced to mine, then the evaporation would be but 4,8 grains; for ſince two hundred and ſixty ſquare inches give ſixteen grains, twenty-ſix ſhould give 1,6 grains; and this quantity multiplied into 3 = 4,8 grains; the difference may proceed from the low ſaturability of the air in his experiments, whoſe effect, as already ſaid, is three times greater than that of temperature, and poſſibly from the attraction of the linen. I have found no obſervations among mine exactly comparable in their circumſtances with his.

THE cold produced by evaporation is an effect deſerving much attention in a meteorological point of view. In the months of July, Auguſt and September, 1783, I made numerous experiments with a view of detecting the connection of this effect with the

the temperature of air in a tranquil ſtate, and the points from which the wind blew, but having no hygrometer I did not examine its ſaturability. The veſſel was the ſame which I employed in meaſuring evaporation already deſcribed; its ſurface twenty-ſix ſquare inches, the depth of the water one inch. The time of expoſure commonly five minutes. The water was ſheltered from the ſun, and almoſt always one or two degrees colder than the air. A thermometer was left in the water and compared with another ſuſpended in the air and ſhade cloſe by it. The experiments were made at two o'clock in the afternoon; I ſhall here ſtate a few:

Bar.

Bar.	Ther.	Wind.	Cold produced in the Water.	Time.
29,9	81°,5	N.	8?	3 minutes.
29.7	81°	NE.	12	5
29,55	79	S.	9	5
29,75	77	E.	6,5	4,5
29,7	77	W.	9,5	5
29,7	74	SW.	7	5
30,01	74	E.	8,5	5
29,9	74	NW.	9,5	5
30,0	72	N.	7	4
29,8	72	ENE.	10,5	5
30,05	72	W.	8,75	5
29,6	66	NNW.	5	5
29,75	65,5	NE.	5,5	5
29,7	61,5	S.	3,5	5
29,85	61,5	N.	3,5	4,5
30,1	56	E.	5	4,5
30,05	53	W.	3,5	5,
30,1	53,5	N.	5	5

HENCE

Hence we ſee, 1o. That when the temperature of the air is from 75° to 80°, and during a calm, the cold produced in water in five minutes is from 8° to 12°, even though the rays of the ſun be intercepted:

2dly, That during the gentleſt breeze from the ſouth the cold is diminiſhed, but if from the north or eaſt, it is increaſed:

3dly, That where the temperature is from 65° to 75°, the cold produced is from 5° to 8°:

4thly, In temperatures from 53° to 65°, the cold produced is from 3° to 5°:

5thly, It is plain that the cold produced is proportioned to the quantity of evaporation in a given time; when this quantity is very ſmall, as twelve grains per hour, or one grain in five minutes, as in temperature 50°, in ſuch circumſtances the cold produced is ſcarcely perceptible; but if the water be expoſed to a current of air, and this air unſaturated, the cold may be more conſiderable.

Sauſsure, as uſual, made ſeveral ingenious experiments on the cold produced by evaporation in agitated air, both on a mountain

where the barometer ſtood at 20 inches, and on a plain where it ſtood at 28,77. It would be tedious to deſcribe the particulars, but the general reſult was, that when the velocity of the wind was about twenty-eight miles *per* hour, or forty feet *per* ſecond, the cold produced by evaporation from a moiſt ſpunge of about three ſquare inches, in which a thermometer was inſerted, was as follows*:

On the Mountain.

Thermr.	Hygromr.	Saturability.	Cold produced.
50°	LVII	6,799 grs.	19°
44,6	LXXXIV	2,328	3°
	On the Plain.		
69°	LI	7,478	20°,5
40°,1	XCI	1,082	2,25

Though various experiments have been inſtituted to aſcertain the cold produced by evaporation in evaporating liquids, yet I know of none wherein the cold produced in air, by the evaporation of liquids in contact with it, has been examined; hence with

* Voy. aux Alpes, § 2060.

with a view of determining this point I have been induced to make the following experiment, May the 25th, 1801, during rain and the wind E.

In a room 13,25 feet high, 24 feet long and 19 broad, whoſe cubic contents of air were therefore ſix thouſand and thirty-three feet, I ſuſpended by hooks from cords 7,5 feet high, and about the middle of the room, two ſheets previouſly wetted, but out of which the moiſture had been ſo far wrung that no drops fell from them, at the diſtance of 4,5 feet from each other, and placed a thermometer between them at the diſtance of about two feet three inches from each, and at the height of five feet, and another thermometer eighteen inches higher than the ſheets, and alſo a thermometer on each external ſide of the ſheets, at the diſtance of two feet on one ſide and eighteen inches on the other.

To eſtimate the loſs of moiſture in the ſheets by evaporation, I cut a ſquare foot of the ſame linen, which weighed, when dry, three hundred and fifty grains, and when wetted, and ſo much moiſture wrung out of it as not to drop, ſeven hundred and twenty-ſeven grains; it was ſuſpended by a wire from the ſcale of a balance.

The ſtate of the inſtruments, before the experiment began, was as follows: hygrometer LXXII. intermediate thermometer 62°, upper thermometer 61,5, lateral thermometers, each 62°.

The experiment began eight minutes before two o'clock, and at eight minutes after two I found the ſtate of the inſtruments as follows: the intermediate thermometer ſtill at 62°, the upper at 61°, and the lateral ones both at 61°. The hygrometer was unfortunately broken during the experiment.

The ſquare piece of linen, in the mean time, (that is during thoſe ſixteen minutes) loſt 23,25 grains of its weight; each ſide of it therefore loſt 11,625 grains. The dimenſions of each ſide of the ſheets were 8,5 feet in length, and 6,5 in breadth =35,25 ſquare feet; hence each ſide of each ſheet ſhould loſe 11,625 grains $\times$ 35,25=409,78 grains. But it is plain from the unvaried ſtate of the intermediate thermometer, that the evaporation from the internal ſides was obſtructed, that from each being obſtructed by that from the oppoſite ſide; and as the upper thermometer was lowered only half a degree while the lateral thermometers were lowered each one degree, it follows that each of the internal ſides evaporated only one-fourth as much as one of the external ſides, and both together half the quantity evaporated by each of the external ſides.

The

The influence of the evaporation of each external ſide of the ſheets extended at leaſt to the diſtance of two feet, as the thermometers indicate; each ſquare foot therefore on one external ſide lowered one degree the temperature of the two oppoſite cubic feet of air, and conſequently the evaporation from 35,25 ſquare feet lowered one degree the temperature of 72,5 cubic feet of air; but theſe 35,25 feet exhaled 409,78 grains of vapour. Therefore the reception of 409,78 grains of vapour in the temperature of 62° cooled one degree 72,5 cubic feet of air, or each cubic foot of air was cooled one degree by the reception and ſolution of 5,65 grains of vapour, and poſſibly by a ſmaller quantity, for I cannot ſay that the cold did not extend farther than the diſtance of two feet.

Now of the hygrometer in the room the temperature being 62°, the complement was therefore ten grains, and as it ſtood at LXXII. the ratio to its complement was 0,64; then as $10 \times 0{,}64 = 6{,}4$ grains, this muſt have been the quantity of vapour which the air originally contained in each of the two cubic feet; to which, if we add 5,65 we ſhall have 12,05, which exceeds the complement by 2,05 grains. It is probable then that theſe 2,05 grains were communicated to a greater diſtance, and that 3,6 grains only remained in each cubic foot. The ſolution therefore of 3,6 grains in a cubic foot of air in ſixteen minutes, and in the

the above circumſtances, cools it one degree. If the ſaturability of the air were greater, it would undoubtedly cool it more, and in a ſhorter time.

LEIDENFROST's experiments on evaporation in very high degrees of heat, ſince verified and extended by Ziegler, Sauſſure, Spalanzani, and D'Antic, and commented on by Lambert, are exceeding curious; but as they have no relation to meteorology, (and though I have myſelf repeated them), I ſhall not introduce them in this eſſay, but paſs to the ſecond object of enquiry relative to the moiſture of the atmoſphere, namely the ſtate of vapour when contained in it.

CHAP. II.

Of the State of Vapour ſubſiſting in the Atmoſphere.

VAPOUR or Moiſture in the atmoſphere may ſubſiſt in denſe air, or in air highly rarefied: that it is found in the former is well known, and that it may ſubſiſt in the latter appears by the obſervations of Bouguer, for he ſaw clouds three or four hundred toiſes above Chiromboracho, and conſequently at the height of twenty-two thouſand five hundred and twenty-eight Engliſh feet, or 4,3 miles over the level of the ſea; a height at which in the temperature of

of 32° a barometer would ſtand at 12,7 inches. At ſuch heights, and at much inferior, ſince evaporation proceeds much more quickly, it is not to be ſuppoſed that all the vapour ſo rapidly produced is diſſolved in the ambient air, but part riſes uncombined as it does under an exhauſted or half exhauſted receiver, and in this caſe Mr. De Luc's ſyſtem is admiſſible. This emiſſion of pure vapour ſeems to begin at heights at which the denſity of the air is 25 (that is at heights at which the barometer would ſtand at twenty-five inches, and thus I ſhall in future expreſs the various denſities of air,) at leaſt it is very conſiderable where the denſity is twenty, as already ſeen, p. 309. This leads me to treat of the properties and ſtate of pure inviſible vapour namely, its ſpecific heat, elaſticity, and ſpecific gravity.

The immortal Doctor Black, the father of all diſcoveries of this kind, informed me that the vapour of water, boiling at 212°, that is at 180° above the freezing point, and poſſeſſing the ſame ſenſible heat as the water, contains nine hundred and forty times more latent heat than an equal weight of water does heated to 212°, or 5,222 times more latent heat than it does of ſenſible heat, counting from the freezing point, for $180 \times 5{,}222 = 940$ nearly. In this caſe the preſſure or denſity of the atmoſphere is thirty, the barometer ſtanding at the height of thirty inches; and with Doctor Black's account the experiments of Mr. Schmidt of Gieſſen

Gieſſen very nearly agree, for according to him the latent heat of the vapour of water, barometer 29,84 inches, and the heat 212°, is 5,33 times greater than its ſenſible heat above the freezing point, now $180 \times 5{,}33 = 959{,}4$*. The difference or exceſs in his experiment proceeds from the preſſure of the atmoſphere being ſomewhat lower, as Mr. Watt's experiments prove.

Mr. Watt diſcovered that the latent heat of ſteam diminiſhed in proportion as its ſenſible heat increaſed, Phil. Tranſ. 1784, p. 335. Now the ſenſible heat of ſteam exceeds 180° above the freezing point when the barometer ſtands above thirty inches, and is leſs than 180° when the barometer ſtands lower than thirty inches, as Mr. De Luc firſt diſcovered, and may be ſeen in Sir George Schuckburg's and Mr. De Luc's tables, Phil. Tranſ. 1779. p. 375. From theſe I have deduced the following table:

*4 Gren's Phys. Journal, p. 315.

Heat

Heat of boiling water.	
Bar.	Heat.
30	212°,
29	210,28
28	208,52
27	206,73
26	204,91
25	203,06
24	201,18
23	199,27
22	197,33
21	195,36
20	193,36
19	191,06
18	188,46
17	185,56
16	184,36
15	180,86
14	176,70

THE accuracy of this table even in the lower part of the ſcale is ſufficiently apparent by the reſult of the experiments of Sauſſure on ebullition on Mount Blanc; for on that enormous mountain, the barometer ſtanding at 16 French inches or 17,05 Engliſh, he found water to boil at the heat of 68°,993 of Reaumur, a degree which on Geneva thermometers is equal to 185°,56 Engliſh.—— Hence we ſee that diſtillation may be more advantageouſly effected on mountains than on plains, and at low barometrical heights than at the greater, yet within certain limits; for at heights that ſurpaſs 8 or 10 thouſand feet, the fuel, by reaſon of the rarity of the air, is more ſlowly conſumed. Hence alſo from the knowledge of the degree of the heat of ebullition to two or more decimal places, the ſtate of the barometer above or below 212° may infered to one or more decimal places.—The reaſon of this rapid diminution of the heat of ebullition below 25 inches is evidently the diminution of reſiſtance, from the diminiſhed weight of the atmoſphere, which then is very ſenſible; but as the cold continually produced by evaporation is then alſo very conſiderable, the *time* neceſſary to procure ebullition is longer as Sauſſure remarked on Mount Blanc. vol. vii. in 8vo. § 2011, p. 328. He found the heat of ebullition barometer 16 to be 68°,993 degrees, or in Engliſh meaſures barometer 17,05. 185°,5 of Fahr. (counting one of Reaumur at Geneva = 2,225 of Fahr.)

Hence ſince, according to Mr. Watt, the ſenſible heats of the vapours of boiling water at different barometrical heights are as the barometrical heights reciprocally, and the ſpecific heats of the vapours of water boiling are as the ſenſible heats reciprocally, it being known, that the ſpecific heat of the vapour of water heated to 180 degrees above the freezing point is 940. The ſpecific or latent heat of the vapour of boiling water, whoſe ſenſible heat is known, (and it may be known by the barometrical height as ſhewn in the above table and the notes) may alſo be diſcovered.

Thus the ſenſible heat of the vapour of boiling water barometer 30 being 180° above the freezing point (212°—32°=180°) and the ſpecific or latent heat of vapour, whoſe ſenſible heat is 208°,56 (that is 176,56 above 32°) as it is when the barometer ſtands at 28 inches, is 958 for ∴ 176,56. 180 :: 940.958*.

As pure inviſible vapour does not in my opinion (of which I have already ſtated the grounds) exiſt in the atmoſphere when its denſity is higher than 25, as it is in moſt of the inhabited parts of the globe, but is always in this caſe united to air, an enquiry into its latent heat at different temperatures below ebullition were ſuperfluous. But as it does exiſt in air whoſe denſity is 25 or leſs, ſince it is found in air whoſe denſity is 12,5, it becomes neceſſary to examine its latent heat in ſuch caſes, in all temperatures

* Hence 169206, being the product of 180°×940, is the common dividend of all ſenſible heats *below* 180°. when the latent heat of the vapour is ſought at barometrical heights below 30 inches.

tures inferior to that of ebullition. Now, by analogy, I apprehend this latent heat in all inferior temperatures may thus be determined:

As the *ſenſible* heat of ebullition, when the barometer is at 25 or below 25 is to the *latent* heat of the vapour at ebullition, ſo is the *ſenſible* heat of water heated to any inferior degree above 32° to the latent heat of its vapour, multiplied by 5,222. Thus the ſenſible heat of water in ebullition barometer 25 being 171°,4 (= 203°,4—32°) its ſpecific heat is 987 (= $\frac{169206}{171,4}$) the latent heat of the vapour of water at 22° above congelation (that is 52° on Fahr. ſcale) is 657 for ÷ 171°,4. 987 :: 22°. 126 × 5,22 = 657. The latent heat of vapour in ſuch caſes cannot be determined by experiment on account of the admixture of atmoſpheric air, we muſt therefore reſort to analogy, which in this caſe is perfect.

The latent heat of pure vapour at greater heights is more conſiderable: thus at heights, at which the barometer ſtands at 20 inches, the latent heat of vapour whoſe temperature is 22° above 32°, as in the laſt caſe, is 730; for the heat of ebullition is 194°8, per table, = 162°,8 above 32°; and the latent heat of the vapour at ebullition is 1039. Now ÷ 162,8. 1039 :: 22. 140 and 140 × 5,22 = 730.

As air is cooled by the reception of moiſture diſſolved in it, we muſt infer that its capacity for containing heat is increaſed; and hence moiſt air is more difficultly heated or cooled than dry air of the ſame temperature. (For the cold proceeds from the abſorption and not from the expulſion of caloric.)

The elaſticity or expanſive force of pure vapour has been examined at every fifth degree of Reaumur above 0 to 110°. by Mr. Betancourt, and may be ſeen in an excellent work of Prony's, his *Architecture Hydraulique*, he has by a moſt ingenious calculation interpolated the expanſions anſwering to the intermediate degrees. But Mr. Schmidt ſeems to have determined this expanſive force ſtill more exactly than Betancourt. Hence I here inſert his table, adding Fahrenheit's for Reaumur's degrees, and diſtinguiſhing the expanſions interpolated by calculation from thoſe actually obſerved by *I.* The forces are meaſured by the elevation of a mercurial column in inches and hundreds of a French inch.*

Reaumur.

*The Paris cubic inch = 1,21 Engliſh. Now the Engliſh cubic inch of mercury when its ſpecific gravity is 13,6 weighs 3443,2 Engliſh grains, therefore the Paris inch weighs 4186 Engliſh grains, and $\frac{1}{10}$ of this Inch = 418,6 grs: and $\frac{1}{100}$ of this inch 41,86 grains.

Reaumur.	Fahrenheit.	Expanſive Force.	Reaumur.	Fahrenheit.	Expanſive Force.
1	34°,25	,01 *I*	21	79°,25	1,01 *I*
2	36,5	,03 *I*	22	81,5	1,01
3	38,75	,05 *I*	23	83,75	1,19 *I*
4	41,	,07 *I*	24	86,	1,29 *I*
5	43,25	,11	25	88,25	1,30
6	45,5	,15	26	90,5	1,38 *I*
7	47,75	,16 *I*	27	92,75	1,42
8	50,	,20 *I*	28	95,	1,60 *I*
9	52,25	,25 *I*	29	97,25	1,80 *I*
10	54,5	,28	30	99,5	1,93
11	56,75	,34 *I*	31	101,75	1,02 *I*
12	59,	,38	32	104,	1,12 *I*
13	61,25	,44	33	106,25	2,23
14	63,5	,50 *I*	34	108,5	2,40 *I*
15	65,75	,55	35	110,75	2,68
16	68,	,61	36	113,	2,80 *I*
17	70,25	,69 *I*	37	115,25	3,20
18	72,5	,76	38	117,5	
19	74,75	,84 *I*	39	119,75	3,40
20	77,	,90	40	122,	3,64
			80	212,	28,

Note.

Note.—1°. Moſt of the interpolations from the 88th degree to the 122nd I have myſelf inſerted as thoſe calculated by Schmidt erred too widely by his own account. 4 Gren Phyſ. Jour. 273.

2do. Mr. Pictet has alſo made a ſet of curious experiments on the elaſticity of pure vapour in low temperatures. *Eſſais de Phyſique* p. 157. He found that a grain of warm water in *vacuo* evaporates in forty minutes in the temperature of 38° Fahr. under a receiver containing 1452 Engliſh cubic inches,* but that it did not diffuſe itſelf equally in leſs than ſix hours, and then raiſed the hygrometer from 17° to 60° that is 43°, and during this whole time the cold under the receiver was conſtantly decreaſing, though ſlowly, which decreaſe undoubtedly contributed to the diffuſion of the vapour.

Mr. Schmidt has alſo made a ſeries of experiments upon the dilatibility of air, made as *dry* as poſſible by expoſure to hot tartarin, an object of great importance, that had never before been examined. This table I here inſert, converting Reaumur's degrees into thoſe of Fahr. and adding from his formula the degrees he omitted.

Expanſion

*Ibid. page 91.

Expanſions of dry air.

Reaumur.	Fahr.	Expanſion of one Inch at 32°.	Reaumur.	Fahr.	Expanſions of one Inch at 32°.
1	34°,25	,0044675		79°,25	,0938175
	36,5	,0089350		81,5	,0982850
	38,75	,0134025		83,75	,1027525
4	41,	,0178700	24	86,	,1072200
	43,25	,0223375		88,25	,11116875
	45,5	,0268050		90,5	,1116155
	47,75	,0312726		92,75	,1206225
8	50,	,035740	28	95,	,1250909
	52,25	,0402075		97,25	,1295557
	54,5	,0446750		99,5	,1340250
	56,75	,0491425		101,75	,1384925
12	59,	,0536100	32	104,	,1429600
	61,25	,0580775		106,25	,1474275
	63,5	,0625450		108,5	,1518950
	65,75	,0670125		110,75	,1563625
16	68,	,0714800	36	113,	,1608300
	70,25	,0759475		115,25	,1652975
	72,5	,0804150		117,5	,1697650
	74,75	,0848825		119,75	,1742825
20	77,	,0893500	40	122,	,1787000
				212,	,3574000

Note.

Note.—1o. Hence we ſee that 1000 inches or meaſures of dry air at 32° would become 1004,4675 at 34.25 Fahr. and at 50° would become 1017,87. Hence 1000 meaſures of *dry* air gain 1,985555 &c. by each degree of Fahr. above 32° (or more compendiouſly 1,9856 which is true to two decimal places) or nearly two.

2do. We ſee the ſource of the diſcordant reſults of D'Amontoms, De Luc, Lambert, Schuckburg, Roy, Berthollet, and Monge, &c. for they all operated upon air more impregnated with various degrees of moiſture; beſides taking the boiling point at different baromtrical heights; in the preſent experiments it was taken at 29,841 Engliſh inches.

3io. It appears that the expanſions are as the differences of heat above 32° as D'Amontons, Lambert and Schuckburg alſo noticed, though their experiments, not being made on perfectly dry air, could not be very exact.

The dilatation of the moiſture contained in air has been ſeparately examined by Mr. Schmidt, and he has ſhewn how from it the volume of air ſaturated with moiſture, ſaturated I ſay at every degree of Reaumur, may be diſcovered; the reſult of his experiments appear in the following table of the volume which 1000 meaſures at 32°

of

of air would acquire if *ſaturated with moiſture* at each degree of Reaumur above 32° expreſſed on Fahrenheits ſcale.*

Reaumur.	Fahr.	Expanſive Force.	Reaumur.	Fahr.	Expanſive Force.
1°	34°,25	1010,56		70,25	1122,68
	36,5	1010,78		72,5	1132,25
	38,75	1016,45		74,75	1142,53
	41,	1022,21	20	77,	1152,83
5	43,25	1028,58		79,25	1164,02
	45,5	1034,97		81,5	1175,23
	47,75	1040,41		83,75	1186,52
8	50,	1048,52	24	86,	1198,59
	52,25	1056,26	25	88,25	1211,44
10	54,5	1064,72		90,5	1223,65
	56,75	1071,28		92,75	1279,62
12	59,	1078,52	28	95,	1377,09
	61,25	1087,11		97,25	1494,02
	63,5	1095,76	30	99,5	1610,02
15	65,75	1104,46		101,75	1725,49
16	68,	1113,21	32	104,	1849,96
			33	106,25	1983,42

*To prevent miſtakes, it muſt be noted that this table is not meant to expreſs the dilatations of air ſaturated at any particular degree of heat it would acquire at other ſuperior degrees, but only the bulk that 1000 parts dry air at 32° would acquire by ſaturation at each higher degree.

Note.—1o. Hence we ſee that air ſaturated with moiſture at high heats is much more expanded than dry air of the ſame temperature, as De Luc and General Roy have alſo obſerved, but in temperatures below 36°,5 dry air is more dilatable which probably induced Sauſſure to conclude it was ſo at higher temperatures. At 54,5 the difference is very perceptible for 1000 parts *dry* air at 32° are expanded at 54°,5 that is by 22°,5 above the freezing point to 1044,67, whereas 1000 parts of air ſaturated with moiſture, are extended to 1064,72 and in higher heats, the differences of expanſion are incomparably greater.

2do Hence it is plain why moiſt air, ſuch as that of the Weſt Indies is much more ſuffocating than dry air of the ſame temperature. For 1000 cubic inches of air ſaturated with moiſture at 86° of Fahr. contain nearly 76 inches of moiſture which is uſeleſs to reſpiration.

3io. Theſe experiments agree with thoſe of General Roy in which ſteam was introduced at hazard, for the General found that from 32° to 52° each degree gave at a mean 2,588 and conſequently theſe 20° would expand 1000 inches to 1051,76, and by Schmidt's experiments much more accurately made, we have 1050,33.

4o. Schmidt

4o. Schmidt alſo obſerved a peculiarity in the expanſion of moiſt air, previouſly noticed by Roy, for Schmidt found that the expanſibility of air, ſaturated with moiſture, was ſmaller than the expanſibility of pure vapour, until the 167th degree of Fahr. but in higher degrees they conſtantly approached nearer to each other. And the General obſerved that the mean rate of expanſion, which from 152° to 172° of Fahr. was 12 for each degree, did from the 172° to the 192° increaſe to 17,88 for each degree, and increaſed ſtill more after the 192d to the boiling point. The ſluggiſhneſs of expanſion of air, ſaturated with moiſture at about 32°, was alſo noticed by the General, and he hence concludes the mean rate of expanſion from 0 to 32° of Fahr. to be 2,27 for each degree, which is ſmaller than that of drier air.

These variations of the rates of expanſibility of moiſt air, ſaturated at different temperatures, Schmidt very juſtly attributes to the variation of the degrees of affinity or adherence of air and vapour to each other at different temperatures. At 32° Fahr. it is very ſtrong, and alſo below that degree; and hence the ſtrong ſolvent power of air, colder than the water it acts upon, remarked by Richman; but if both are equally cold very little moiſture will be taken up by the air, as already mentioned; and hence I have ſaid that air diſſolves vapour when this is in a naſcent ſtate.

But in heats above 167° or 170° air and vapour are diſpoſed to ſeparate.

5°. HENCE we may deduce the impoſſibility of diſcovering a coefficient univerſally applicable to expreſs the rate of expanſion of air in every ſtate of moiſture, (as Tremley has well noticed. See 2 Sauſſure voy. aux Alpes 4°). This muſt vary with the mean ſtate of hygrometers above and below the heights to be meaſured; and experiments of this kind have not yet been made. De Luc's coefficient anſwers tolerably well for very dry air, that is whoſe ſaturability is greateſt, Sir George Schuckburg's for air much moiſter, and General Roy's for air ſtill more moiſt, that is whoſe ſaturability is ſmalleſt. Hence each ſucceeds in certain caſes, and fails in others. The dilatation or contraction, which air ſaturated with moiſture at any one given degree of temperature receives without the addition of any more moiſture, at any higher or lower degree of temperature, has not as yet been diſcovered; for Schmidt, who alone has attempted it, is juſtly diffident of the correctneſs of the table he has given of it, and in fact it is not grounded on the indication of any known hygrometer, and improperly ſuppoſes the L. degree to indicate the mean betwixt the loweſt and ſaturation. Whereas the LXVth degree on Sauſſure's indicates that mean; and XCVIII. and not C. indicates ſaturation.

ACCORDING

ACCORDING to Mr. Watt (as ſtated by De Luc, 3 Meteorolog: p. 145) the ſpecific gravity of pure vapour is to that of air as 4 to 9. I ſuppoſe he compares it with air at the uſual denſity of 30 or 29, and at ſome particular temperature which is not mentioned; for at high temperatures the difference muſt be much greater, as appears by the foregoing tables.

MR. SAUSSURE, *Hygrometer*: p. 284, has given us the ſpecific gravity, not indeed of pure vapour, but of vapour diſſolved in air, with more preciſion, for he tells us, 1o. That a cubic foot of perfectly dry air has its volume augmented by $\frac{1}{14}$ when ſaturated with ten grains of moiſture at about 65° Fahr. of heat, and barometer 28,77 inches (Engliſh).

2do. THAT a cubic foot of *pure* or perfectly dry air of that denſity and at that temperature weighs 751 grains (French), and after diſſolving 10 grains of moiſture, by which it is dilated $\frac{1}{14}$, this new volume weighs $751 + 10 = 761$ grains; but a cubic foot of *pure* air augmented by an acceſſion of $\frac{1}{4}$ of its bulk of pure air would weigh $751 + \frac{1}{14}\,751 = 765$ grains, that is 14 grains more. Hence he infers that in this caſe the ſpecific gravity of the diſſolved moiſture is to that of dry air as 10 to 14, for $\frac{1}{14}$ of a cubic foot in the one caſe weighs 10 grains, and in the other 14 grains nearly.

BUT

But I ſtrongly ſuſpect that the original experiment, on which this calculation is founded, is erroneous, chiefly by reaſon of the ſtrong adherence of moiſture to cold glaſs, as will hereafter be ſeen in treating of dew. From Schmidt's experiments, it may be inferred, that the ſpecific gravity of vapour diſſolved in air at this temperature is much lower with reſpect to that of pure air than Sauſſure has ſtated, for he tells us that about 1066 meaſures of dry air in temperature 65° would, if ſaturated with moiſture at that temperature, occupy the ſpace of about 1100 meaſures, and conſequently receive an augmentation amounting to about $\frac{1}{32}$ of their bulk; now, transferring this ratio to the cubic foot in Sauſſure's experiment, it appears that $\frac{1}{32}$ of a cubic foot thus added to the cubic foot of dry air weighs 10 grains; but a cubic foot of dry air, augmented by an acceſſion of $\frac{1}{32}$ of ſimilar air, would weigh 751 + 23,46 grains, which approaches nearly to Mr. Watt's ratio, therefore the ſpecific gravity of vapour diſſolved in air at this temperature is to that of perfectly dry air as 10 to 23,5 nearly. It ſhould however be recollected that Mr. Sauſſure found that a cubic foot of dry air in reality took up 11,069 grains of moiſture when ſaturated at this temperature, and that it was only by way of conceſſion to thoſe againſt whom he argued, that he ſtated the weight taken up at 10 grains; then we ſhould have of 11,069 to 21,195, or in round numbers as 11 to 21 or 10 to 19. And it ſhould farther be remarked, that the temperature is given very

loofely,

looſely, for it is ſtated to be from 14 to 15 or 16 degrees of Reaumur. See *Hygrometer*: p. 104 and 284.

Sauſsure has given us a table, by the help of which the abſolute quantity of vapour at any barometrical height, in a cubic foot of air being known, the proportion and abſolute quantity in a cubic foot, at another barometrical height 3,6 inches lower, may be known from the mercurial height 28,77 to that of three inches and one-half, nearly.

This table I here give, adapting it to our meaſures.

Barometer.	Ratio.
28,77	1,0000
25,17	0,9528
21,57	0,8899
17,97	0,8264
14,37	0,7629
10,77	0,6887
7,17	0,6230
3,57	0,4311

Thus ſuppoſing the abſolute quantity of diſſolved vapour at any temperature, and barometer 28,77 to be 10 grains per cubic foot, then the quantity of vapour at a height at which a barometer would ſtand at 25,17 inches would be $10 \times 0{,}9528 = 9{,}528$ and at the height at which a barometer would ſtand at ſeven inches, the quantity in a cubic foot would be only $10 \times {,}6230 = 6{,}23$. But ſtill it is ſuppoſed that at thoſe great heights, at which barometers would ſtand ſo low, that the air is of the ſame temperature as the original experiment

riment is made at, namely in this caſe, as it is found at barometer 28,77 inches; but ſince in reality air at great heights is generally much colder than below, to aſcertain the real proportion of vapour at thoſe heights it will be neceſſary to find the quantity of vapour which a cubic foot of air is capable of holding at that temperature barometer 28,77, and the ratio which the quantity or weight of vapour actually found bears to the complement at that temperature. Then, 2do. to find the complement of a cubic foot of air at the temperature which prevails at the given barometrical height, and diminiſh it in the ſame ratio in which it was found diminiſhed below, and finally diminiſh it ſtill farther in the ratio which that barometrical height demands. An example will fully explain this rule.

Thus Sauſſure found, barometer 28,77 and thermometer 82° of Fahr. a cubic foot of air contained about 10 grains of moiſture at Geneva. Now the complement of 82° is nearly 15 grains, and the ratio of 10 to 15 is $\frac{2}{3}$. Then at Mount Blanc, on the ſame hour, the barometer ſtood at 16° and the thermometer at 26°,8; the complement of a cubic foot of air at this temperature is 5,3 grains, which diminiſhed in the ratio of 2 to 3 becomes 3,5, and this, farther diminiſhed by the ratio which the barometrical height of 16 inches demands, namely $,78 = 3.5 \times ,78 = 2,7$ grains, by obſervation it was found to be 1,7; the difference is only

only one grain. Voy aux Alpes, § 2007. How the temperature which prevails at thoſe great heights may be found, will be ſhewn in the ſequel.

The celebrated Lambert, of Berlin, Mem. Berlin, 1772, has alſo given an eſtimate of the proportion of vapour which prevails in the atmoſphere at different barometrical heights, deduced from calculations founded on many fictions, ſuch as that of an homogeneous atmoſphere, of pure air diſtinct from common air, and an erroneous ſyſtem of the aſcent of heat; yet as it is much eaſier in its application and in the inſtance juſt quoted, approaches very near the truth, I have calculated the reſults of his ſyſtem, which is nothing more than that the quantity of vapour at different barometrical heights above the earth is in the ratios of the ſquares of thoſe heights. By an homogeneous atmoſphere it is probable he meant ſuch a ſtate of the atmoſphere as prevails in ſerene unclouded weather, and it is certainly only in ſuch an atmoſphere that any calculation can be inſtituted.

Table of the Ratios of the Quantities of Vapour at different barometrical Heights, the Quantity at the Surface of the Earth being given.

Barometer.	Ratio of Vapour.	Barometer.	Ratio of Vapour.	Barometer.	Ratio of Vapour.
30,	900	24,	576	12	144
29,5	870	23,	529	11	121
29,	841	22	484	10	100
28,5	812	21	441	9	81
28,	784	20	400	8	64
27,5	756	19	361	7	49
27,	729	18	324	6	36
26,5	702	17	289	5	25
26,	676	16	256	4	16
25,5	650	15	225	3	9
25,	625	14	196	2	4
24,5	600	13	169	1	1

Thus in the example laſt quoted, the quantity of vapour in a cubic foot at Geneva being 10 grains, barometer 28,77, the quantity

quantity on Mount Blanc, barometer 16, ſhould be ,309, for as 827,7 (= $\overline{28,77^2}$) is to 256 (= $\overline{16^2}$) ſo is 10 to 0,309, which differs from the truth by only 0,391 of a grain.

As vapours unite to air, partly through the agency of heat, and partly through that of affinity and of electricity, ſo they ſeparate from it, ſometimes from a diminution of that degree of heat which they poſſeſſed in their naſcent ſtate, ſometimes from a diminution of affinity, and ſometimes from an alteration in their electrical ſtate.

In their firſt degree of coaleſcence when ſeparated from air, they form aggregates of exceeding minute particles, ſeparated from air by the diminution of affinity, and alſo from each other by electrical atmoſpheres; theſe aggregates are of equal, and often lower, ſpecific gravity, than the air in which they are formed, and yet are viſible by reaſon of their opacity; when near the earth they are called *foggs*, *miſts* or *haze*, (which differ only in denſity) and when at greater heights, *clouds*.

Vapours iſſuing from water or moiſture warmer than the air to which they unite, are ſoon cooled by it and thence in great meaſure diſmiſſed; hence the morning miſts obſerved in ſummer and the winter miſts of the colder regions; evening miſts on the

contrary proceed from the ſuperſaturation of air with vapours previouſly diſſolved, ariſing from the ſupervening decreaſed temperature. The inferior ſtrata of the atmoſphere are ſcarce ever ſuperſaturated by vapours ariſing from water or moiſture warmer than the air into which they aſcend; for before the point of ſaturation can be attained, their affinity to the portion of air to which they are united is weakened, and thence exceeded by the unincumbered affinity of the ſuperior ſtrata, and this happens ſucceſſively on to the higher regions; but with diminiſhed activity, by reaſon of the diminiſhed denſity of the higher ſtrata, until their ulterior progreſs is checked by ſaturation; but as they are ſtill continually recruited from below, their quantity is at laſt ſo far increaſed that they coaleſce into clouds. Here the proceſs recommences, for from the ſurface of theſe clouds a freſh evaporation often takes place, which, after ſome progreſs, is again checked in its turn, and clouds are formed at a ſuperior height; theſe again give room to a further evaporation, and a new ſtage of clouds is formed, until the proceſs is at laſt arreſted by the intenſe cold of the ſuperior regions. But the mere cold of congelation is not ſufficient to arreſt it, for Bouguer informs us that clouds are formed 2500 feet above the lower line of congelation, and that ice itſelf evaporates, though cooled, ſeveral degrees below the freezing point, is well known. The diſtance of the particles, both of air and vapour, from each other, when ſo far rarefied as they

they muſt be in the ſuperior regions of the atmoſphere prevents their coaleſcence in any but the extreme degrees of cold.

Hence we ſee that in the warmer latitudes and ſeaſons, various ſtrata of clouds may be formed one above the other; Muſchenbroock atteſts, that even in Holland, in Auguſt 1748, he diſtinctly diſcerned three. Theſe diſtinct ſtrata, variouſly electrified and otherwiſe circumſtanced, give occaſion to various phænomena, the detail of which would here be miſplaced.

The clouds which commonly crown the ſummits even of low mountains, and often announce rain, are cauſed by the near approach to ſaturation at thoſe elevations, and its actual attainment through the evaporation from thoſe ſummits. But the ſummits of the loftieſt mountains ever crowned with ſnow, are generally ſhrouded in clouds from the cold they impart to the air in contact with them, and the loſs of electricity conducted away from the vapours contained in that air, by the mountain.

The heights at which the loweſt clouds are formed are various in various latitudes and ſeaſons; greater in the warmer and ſmaller in the colder. In latitude 54° in Cumberland, Mr. Croſthwaite obſerved none lower than 2700 feet, and none higher than 3150 in

in the courſe of ſeveral years*. But this country being mountainous they are probably lower than in others under the ſame parallel. Lambert, in Berlin, latitude 52°.32′, in the month of July 1773, found their height 7792 feet, thermometer 65°, and the barometer ſomewhat below its mean height†. Schuckburg alſo remarks, that clouds frequently reſt below the ſummit of *Saleve*, whoſe height is 2831 feet. Phil. Tranſ. 1777, p. 538, and Gentil at Pondicherry, latitude 12°, obſerved ſome at the height of 10240 feet. 2 Voy. p. 79.

The weight of clouds, Sauſſure eſtimates at one-third or one-fourth of that of the cubic foot of air in which they ſubſiſt. Hygrometer. p. 270. When the barometer riſes, clouds are partly diſſolved, as denſe air is a better ſolvent than rarer air, and partly riſe higher in conſequence of the increaſed ſpecific gravity of the inferior air; when the barometer falls the contrary takes place.

CHAP.

* D'Alton's Meteorological Obſervations, p. 41.

† Mem. Berlin, 1773, p. 44.

CHAP. III.

Of the Temperature of the Atmoſphere.

SECTION I.

Of the Temperature of the Summer Months.

THAT the heat of the atmoſphere is derived, not from the immediate action of the ſolar rays that traverſe it, but from the warmer and more ſolid bodies with which it is or has been in contact, is a fact which at preſent can admit of no diſpute. I have ſuſpended a thermometer and a feather about an inch over the focus of a moſt powerful burning lens, without producing the ſlighteſt increaſe of heat in the one or of motion in the other. Hence it is plain the air was no way affected by it. That the heat communicated to the atmoſphere is in general gradually diminiſhed in ſome proportion during its progreſs upwards, at leaſt in ſummer, is alſo a fact ſufficiently aſcertained by the teſtimony of thoſe who have aſcended to great heights, either on mountains or in baloons. But the ratio in which heat is diminiſhed in its progreſs upwards, has been variouſly ſtated, and the means by which this diminution is effected, appear to me to have been incorrectly determined.

EULER

Euler ſuppoſed heat to decreaſe during its aſcent in air in an harmonic progreſſion; but this opinion has been long abandoned. Lambert, in the Mem. of Berlin for , fixes the ratio of this decreaſe to be that of 17 to 12; but this aſſertion is refuted by Sauſſure, Voy. aux Alpes, § 929, and is indeed incompatible with the reſults of experiments made at different heights, meaſured barometrically. Sauſſure, Voy. aux Alpes, § 2051, thinks that in latitudes from 45° to 47°, heat decreaſes during its aſcent 2°,25, or more correctly 2°,23 Fahr.* degrees for every 640 Engliſh feet, during the ſummer months, from the level of the ſea to the ſummits of the higheſt mountains, and conſequently one degree of Fahr. for each 287 feet of height. Yet this determination, though limited within ſuch narrow bounds, is not always juſt, even within thoſe bounds, and frequently errs very widely, at leaſt when the diminution of heat is computed, not from the level of the ſea, which frequently cannot be known, but from that of adjacent plains, to the ſummits of mountains. According to this rule, the diminution is conſtant and invariable, let the heat below be ever ſo different, as the diminution is proportioned to the height only, which does not vary. But numerous obſervations atteſt the diminution of heat to vary almoſt every day and hour, thus Schuchburg found the diminution of heat on Mount Saleve on

* The boiling point of water being taken at Geneva when the barometer was at 28,77.

on one day 9°, on another 8°,9, on another 10°,5; whereas, calculating from the above rule, it ſhould always be 9°,9; ſo on the Mole he found the diminutions of heat on different days 7°, 5°,8, 6°,6, 7°,9; whereas, by the above rule, it ſhould be 14°,8 every day. Here the error amounts to the double, or more.

To attain a cloſer approximation, we muſt reſort to ſome fixed point above, whoſe temperature is known, and ſo diſtant, that its variations, if it undergoes any, ſhould, with reſpect to that diſtance, be inconſiderable; then, this diſtance being known, and having another fixed point below, namely, the degree of heat obſerved, and framing a ſet of equi-diſtant intermediate terms, we have the neceſſary elements of a decreaſing arithmetical progreſſion, which all obſervations concur in eſtabliſhing as that obſerved by heat in its progreſs upwards, during the ſummer months, and in ſerene weather.

The fixed point in the ſuperior region of the atmoſphere to which I here allude is the height of the upper term of congelation. This, as well as the lower term of congelation, was firſt announced by Bouguer, ſo long ago as Ann. 1749, though no uſe has ſince been made of either in meteorological enquiries.

The *upper* term of congelation is that above which no *viſible* vapour aſcends, and its temperature is *conſtantly* at leaſt 32° of Fahr. Bouguer has ventured to lay down its height over the ſnowy mountains of Peru under the equator, but it is highly probable that over the ſea, and eſpecially over land much heated, its height is ſtill greater within the ſame latitudes, ſince its height is proportioned to the intenſity of the heat below. And hence, between ſun-ſet and ſun-riſe, if the heat below be much diminiſhed, it muſt deſcend in proportion to the diminution. Bouguer has determined its height only in a ſingle inſtance, but its importance has induced me to inveſtigate the variations of this height in all latitudes above latitude 5°. The principles which I employed in this inveſtigation may be ſeen in the *Eſtimate of the temperature of different latitudes*, publiſhed in 1787, p. 8. Since that time I have corrected the heights appropriated in that eſſay to latitude 55°, and every other 5° higher, in the northern hemiſphere, by taking the inverſe ratio of the height in latitude 45°, divided by the ſine of 30°, namely, 2. The intervals I have filled by taking arithmetical means. Without the tropics, it is evident that not only night and day, but alſo winter and ſummer, muſt vary this height, and, in the higher latitudes, very conſiderably; hence I have confined this determination to the ſummer months, namely, May, June, July, and Auguſt, without the tropics. During the winter months, October, November, January, and February, this line

line muſt frequently approach nearly to the lower line, yet they never coincide, or at leaſt very rarely, namely, when the cold of the lower air is ſo great that no viſible vapours are formed in it. In the month of April, and in part of that of October, this line is intermediate between its height in ſummer and that in winter. And during the whole year it muſt be taken as a mean between its heights over the oceans and over land.

The *lower* term of congelation is ſituated at the height at which it freezes at night, though in day time, and particularly on ſunny days, it may ſurpaſs that point by ſeveral degrees within the tropics; though fewer without them, even in the ſummer ſeaſon. Thus Bouguer found the heat on the ſtony ſummit of Pinchinca, at whoſe height he fixes this term, to vary 17° of Reaumur, that is 38° of Fahr. from ſun-riſe until noon. Voy. p. xxxix.

He alſo ſtates the height of this line in latitude 28° at 13240 feet, and Sauſſure, more correctly, at about 12170 feet; and I believe it ſtill lower.

In the latitude of the ſouth of France, (that is 43° and 44°) Bouguer ſtates the height of this line to be about 10240 and 9600, but Sauſſure eſteems it lower, for he makes it between latitude 42° and 43°, (that of Canigou) to be only 9290 feet*.

* Voy. aux Alpes, p. 374.

All theſe determinations muſt neceſſarily vary according to the ſeaſons, and according as the heights are computed perpendicularly to ſea or land, nay even according to the diſſerent ſituations of the land and its different temperatures.

Thus, in the German or Northern ſide of Swiſſerland, its height is only 8312 feet, but on the Southern or Italian ſide, not above ninety miles diſtant, it reaches 8952 feet*. The height of *Col de Geant* ſurpaſſes the height of this line, and yet the thermometer roſe 10° Fahr. about two o'clock†, but at night it fell beneath that line.

These variations render the *lower* line of congelation an unfit limit towards which the diminution of heat ſhould be computed, and hence I fix the limit on the *upper* line of congelation, whoſe variations are, reſpectively, at leaſt in ſummer and ſerene weather, much leſs conſiderable.

Table

* Bertrand, Renouvellementdes terres, 251.
† Sauſſure, Voy. § 2051.

Table of the Height of the Upper Line of Congelation in the different Latitudes of the Northern Hemisphere.

N. Latitude.	Feet.	N. Latitude.	Feet.	N. Latitude.	Feet.
0	28000	21	24404	38	17393
5°	27784	22	24147	39	16801
6	27644	23	23890	40	16207
7	27504	24	23633	41	15712
8	27364	25	23423	42	15217
9	27224	26	22906	43	14722
10	27084	27	22389	44	14227
11	26880	28	21872	45	13730
12	26676	29	21355	46	13235
13	26472	30	20838	47	12740
14	26268	31	20492	48	12245
15	26061	32	20146	49	11750
16	25781	33	19800	50	11253
17	25501	34	19454	51	10124
18	25221	35	19169	52	8965
19	24941	36	18577	53	7806
20	24661	37	17985	54	6647

N. Latitude.	Feet.	N. Latitude.	Feet.	N. Latitude.	Feet.
55	5617	67	4616	79	3911
56	5533	68	4548	80	3861
57	5439	69	4480	81	3815
58	5345	70	4413	82	3769
59	5251	71	4354	83	3723
60	5148	72	4295	84	3677
61	5068	73	4236	85	3631
62	4989	74	4177	86	3592
63	4910	75	4119	87	3553
64	4831	76	4067	88	3514
65	4752	77	4015	89	3475
66	4684	78	3963	90	3432

The height of this line in any latitude, divided by 100, gives the number of terms in the arithmetical progreſſion, each conſiſting of 100 feet; and the difference of the heat below, and 32°, gives a dividend, which, divided by the number of terms, leſs one, gives the common difference of the progreſſion*; and the common difference, multiplied by the number of terms in the height of a mountain,

* It is commonly a decimal fraction, of which the three firſt ſignificative figures are generally ſufficient, but it is ſafer to take four in ſome caſes.

mountain, or other height ſought, gives the diminution of heat at that height; and this diminution ſubſtracted from the heat below, gives the degree of heat that obtains at the propoſed height.

EXAMPLE.

In latitude 56°. 30′, the heat below being 54°, it is required to know the diminution, and the degree of heat that prevails at the height of 1364 feet?

Here the latitude being intermediate between latitude 56° and 57°, the height of the upper line of congelation is the mean between 5533 and 5439 = 5486 feet, then the diviſor is 53,86 terms, and the dividend is $54° - 32° = 22$, and $\frac{22,00000}{53,86} = ,408$, the common difference. Now $0,408 \times 13,64 = 5,5$ the diminution, and $54 - 5,5 = 48°,5$, the degree of heat which prevails above; which is exactly that obſerved by General Roy at the ſummit of Knockfarle in Scotland. Phil. Tranſ. 1777, the heat below being 54°, and the height 1364 feet.

In this manner I have calculated the heats at various heights in various latitudes, the degrees below being given, the reſults both by obſervation and calculation may be ſeen in the annexed table.

Table

Table of the Heats observed and calculated on ſundry Mountains.

Latitude	Heights.	Heat			Difference	Authors.
		Below.	Above by Obſervation.	Above by Calculation.		
0	Pinchinca 15564	83°,75	29°,75	30°,5	0°,75	Bouguer.
38°	Etna 10954	73,25	40,08	47,2	6,4	Sauſſure, Voy. 941.
46°	Saleve 2831	73,	64,	64,16	0,16	Schuck. Phil. Tran. 1777, p. 530.
46°	Mole 4211	63,9	56,	53,7	2,3	Schuckburgh.
46°	Col de Geant 11273	81,	41,5	42,5	1,0	Sauſſure, § 2035.
46°	Mt. Blanc 14624	82,8	26,82	26,44	0,38	Sauſſure, § 2003, p 307
53°	Snowden 3555	60,	47,75	47,1	0,65	Roy, Phil. Trans. 1777, p. 779.
53°	Moel Elio 2371	62,5	51,	51,3	0,3	Roy, ibid.
56°	Arthur's Seat near Edinburgh 803	54,	50,5	50,	0,5	Roy, ibid, p. 777.

Latitude.	Heights.	Heat			Difference	Authors.
		Below.	Above by Obſervation.	Above by Calculation.		
56°	Kirk Yetton Cairn 1544	54°,5	47°,25	48°,3	1°,05	Roy, Phil. Trans. 1777, p. 777
56°,30′	Schihallien 3281	56,	5,	41,4	3,6	Roy, ibid.
56°,30′	Bolfrack 1076	60,	56,75	54,5	2,25	Roy, ibid.
56°,30′	Knockfarle 1364	54,	48,5	48,5	0,	Roy, ibid.
56°,30′	Glenmore 1279	55,	51,5	49,6	1,9	Roy, ibid.
56°	Tinto 1642	58,	51,	50,3	0,7	Roy, ibid.
46°	Dole 4810	78,25	65,	63,44	1,5	Roy, ibid, 783 & 736.
51°,31′	Kew Pagoda 116	49,25	49,25	49,05	0,2	Roy, ibid, 773.
80°	Hacklyyt Hill 1503	50,	42,	43,	1,	Phips' Voy.

HERE are eighteen calculations of heat at different heights, under different latitudes; among which only two differ remarkably from the heats actually obſerved at thoſe heights, and theſe I am inclined to attribute to an error in the obſervations, for the heats above and below cannot both be noted by the accurate philoſophers who meaſure the heights of mountains. The errors, if any are committed, in noting the degrees of Reaumur, are much more conſiderable than thoſe that can be committed in taking thoſe of Fahrenheit.

THE near agreement of the calculated and obſerved heat on the ſummit of Mount Blanc is very remarkable, as the height of this mountain ſurpaſſes that of the upper line of congelation, and is the only known height that does ſo; that it ſurpaſſes the upper line of congelation is evident from Sauſſure's account; for he tells us that the air was ſo free from vapour on this *ſummit*, that in a ſhaded ſpot he could ſee the ſtars by day, and the temperature even in day time was far below 32°; the ſnow that nevetheleſs repoſed on its ſummit might for the moſt part have been drifted on it by the furious winds that rage in winter. Thus Dr. Villars found thirty beds of earth interpoſed between thoſe of frozen ſnow on the Glaciers of Swiſſerland. 22 Rozier, p. 275. This earth or duſt was wafted up in the months of July and Auguſt during thirty ſucceſſive years.

THE

The clouds that frequently inveſt its ſummit originate from vapours condenſed by the abſorption of electricity by the mountain. It is alſo highly remarkable that the common difference of the progreſſion ſhould ſuit terms beyond the laſt, by the means of which it was found. It would therefore ſeem that the common difference thus found takes place at much ſuperior heights.

The ſmaller differences between the degrees of heat calculated, and thoſe obſerved, may well be attributed to local circumſtances; *below*, in ſome ſituations of ſmall extent, *reflected* heat often occurs; *above* if the ſummit be graſſy the heat is diminiſhed in ſome degree by evaporation; if ſtony but narrow, the genuine heat is little affected; but if broad, as the ſtony ſummit of Pinchinca, a conſiderable degree of heat may ariſe from reflection, on ſunny days. Darker days ſeem more favourable to detecting the true heat appropriated to the height.

Notwithstanding the imperfections incident to obſervations on mountains, yet theſe are upon the whole much leſs exceptionable than thoſe that hitherto have been taken in balloons, of which the height is ſeldom determined with ſufficient preciſion; beſides ſimultaneous obſervations of the temperatures above and below rarely occur, and on ſunny days the heat reflected by balloons cauſes conſiderable errors.

Lastly, it ſhould be noticed that the horizontal diſtance between the places of obſervation ſhould not exceed eight or ten miles.

By means of this progreſſion ſeveral curious queſtions may be reſolved, the latitudes being given.

Let H denote the heat below, h the heat above, D the diminution, d the common difference or rate of the progreſſion, n the number of terms in the propoſed or ſought for height, which multiplied by 100 gives the height truly, to leſs than 20 feet.

L the height of the upper line of congelation, in the given Latitude.

Then 1° $d = \frac{H-32}{L-1}$. 2do. $D = d\,n$. 3io. $n = \frac{D}{d}$. 4°. $H = h + D$

5° $h = H - D$. 6° $L = (\frac{H-32}{d} + 1) \times 100$.

Thus 1°. the heat below being given in latitude 56°,30′ to find the rate of its progreſſion upwards? Suppoſe the heat below 54°, then by the firſt formula $\frac{54-32}{54,86-1} = \frac{22}{53.86} = ,408$; then it is diminiſhed ,408 of a degree in every hundred feet of its progreſs upwards.

Thus

THUS in the firſt hundred feet it becomes $54° - ,408 = 53°,592$. And at 200 feet it becomes $54° - \overline{,408 \times 2}$, &c.

2do. THE *rate* of diminution being known, ſuppoſe ,408 to find its amount at a given height, ſuppoſe 1364 feet? By the ſecond formula we have $0,408 \times 13,64 = 5°,5$.

3io. THE diminution of heat being known, ſuppoſe $5°,5$, and alſo its rate, ſuppoſe 0,,408, to find the height at which this diminution takes place? By the third formula we have $\frac{5,5}{0,408} = 13,48$ which $\times 100 = 1348$ feet, which wants but 15 feet of the truth.

HENCE we ſee that heights may often be meaſured by thermometors only with tolerable accuracy.

4o. THE heat *above*, ſuppoſe $48°,5$, and its diminution, ſuppoſe $5°,5$ being given, to find the heat *below*? By the fourth formula $48°,5 + 5°,5 = 54°$.

5°. So alſo the heat *below*, and the diminution above, being known, the heat *above* may be found by the fifth formula as is evident. Thus $54°$ (in the laſt example) $- 5,5 = 48,5$.

6°. THE

6°. The heat *below* and the rate of progreſſion being known, the height of the line of congelation may be found by the ſixth formula. Thus in the example $\frac{54-32}{40846} + 1 = \frac{22,00000}{40846} + 1 = 53,86 + 1 = 54,86 \times 100 = 5486$.

Note.—That the heat *below* and the heat *above* being given, if the difference includes no fraction, and if the height alſo be known, the rate or common difference may be found by dividing the difference of heats above and below, by the height or $d = \frac{H-h.}{n}$ n being expreſſed in the terms of the progreſſion. Thus the height of Tinto being 1642 feet it ſhould be expreſſed 16,42.

Some have thought that heat is conveyed upwards by the gradual aſcent of the lower ſtrata of air which firſt receive the increaſe of heat, but that this cannot happen will be ſhewn in the ſequel.

In ſerene weather, whatever variations may occur in the temperatures below, the differences between them and the heats above are as nearly the ſame as can be expected, when due allowances are made, for the imperfection of thermometers on which fractions

tions of degrees are often had only by eſtimation, and alſo for local circumſtances.—A full proof that heat aſcends in an arithmetical progreſſion.

Thus in the three ſeries of obſervations on the temperatures at the top and bottom of *Mount Saleve*, the difference in the firſt was 8°, 9, in the ſecond 9°, in the third 10°, 5, here the greateſt deviation is 1°,5.

And in the ſixth ſeries at the *Mole* the differences were, in the firſt 7°,1, in the ſecond 5°,8, in the third 7°, in the fourth 7° 9, in the fifth 7°, in the ſixth 6°,6. Here two circumſtances deſerve attention; firſt, that in the ſecond, third and fourth ſeries the heat above was conſtant, namely 56°, while the heat below varied from 61°,8 to 63°'9. This may be attrbuted to a briſk wind, from the glaciers which reigned above, while the lower flank of the hill was ſheltered. The ſecond is, that the difference between the heat abvoe and the heat below, with reſpect to Mount Saleve was 9°, and yet betwixt the top and bottom of the Mole it was at moſt only 7°9 though the height of Mount Saleve was only 2831 feet and that of the Mole was 4212. This muſt certainly proceed from the intervention of ſome contingent cauſe, the general difference may therefore be often better determined by calculation than by actual obſervation.

In

In a *given latitude*, the rate of diminution at equal heights, is greater when the temperature below is high, than when it is low; for while the divifor (viz. the height of the line of congelation) remains unaltered in all temperatures, the dividend (viz. the difference betwixt the heat below and 32°) is fo much the greater as the temperature is higher, and confequently the rate or quotient muft be higher. Sauffure alfo noticed this difference of the rate of diminution in high and low temperatures; for he tells us that in winter the rate of diminution is but $\frac{1}{3}$ of that which it amounts to in fummer. Voy. § 2051. And between the tropics heat rifes much more rapidly than in the higher latitudes, as Dr. Caffan obferved, 36 Roz. Jour. p. 265. See alfo Mr. Six's obfervations in the Phil. Tranf. 1788, p. 105. And hence the difference betwixt the temperature of mountains and of plains is not fo great in winter as in fummer. Nay in winter the temperature at great heights is often warmer than on plains, as will prefently be fhewn.

We owe to Mr. Pictet fome new and curious experiments on the progrefs of heat at low heights; that is between 5 and 75 feet. He found that a thermometer, fufpended about five feet over the earth, generally increafed in heat more rapidly than a thermometer placed 75 feet over the earth; fo that on the hotteft hour of the day, the lower was often about 4,5 degrees warmer than the upper thermometer. This fhews that the increafe of heat below is quicker than its communication upwards. However

towards

towards ſun-ſet both thermometers approach to an equality. This ſhews that the higher thermometer at 75 feet communicates its heat upwards ſtill more ſlowly than the lower communicates its heat to it; which proves that the higher heat aſcends, the more ſlowly it is propagated to ſtill ſuperior heights. This explains the conſtancy of the heat at the ſummit of the mole while the heat below increaſed as already mentioned. But after ſun-ſet the lower thermometer cools quicker than the upper, and towards the end of twilight is often about four degrees colder, notwithſtanding that the earth a little below its ſurface is much warmer. The cold which thus affects the lower thermometer is evidently cauſed by evaporation; this exceſs of heat of the upper over the lower thermometer laſts the whole night, and until two hours after ſun-riſe, when, notwithſtanding the evaporation, the earth is more heated by the ſun's rays than it is cooled by evaporation, and ſo alſo is the thermometer ſuſpended five feet over it, and thus accumulates heat faſter than it can reach the upper thermometer, until after the hotteſt hour of the day, as already mentioned.

These obſervations happily point out the hours of the day moſt proper for diſcovering the mean temperature of the atmoſphere, a matter of great importance in barometrical menſuration. It is evident that the hour of ſun-riſe (or before ſun-riſe) is an improper time, ſince the heat at the height of 75 feet is ſome

degrees higher than that at five feet above the ſurface of the earth; contrary to the grounds of calculation. Nor is the hotteſt hour of the day the fitteſt time, ſince the heat is above all proportion ſome degrees greater than at the height of 75 feet. The propereſt times are therefore the mean hours between ſun-riſe and the hotteſt hour of the day*, and between this and ſun-ſet. *Eſſais des Phys.* § 131, &c.

However, ſuch conſiderable diſparities occur only in calm, clear and ſerene weather; on cloudy days, when the ſun does not appear, they vaniſh. Ibid. § 135.

Whenever a fog or miſt appears in any ſtratum of air, it communicates about a degree, or half a degree, of heat to the ſtratum in which it is found. Ibid. § 142. A circumſtance that deſerves notice in eſtimating the mean temperature of the atmoſphere. Mr. James Six, of Canterbury, made a number of obſervations on the temperatures ſix or nine feet above the earth; and alſo on that which prevailed 220 feet above it; and his reſults were in general analogous to thoſe previouſly obtained by Pictet. But the temperatures of whole days, or of days and nights only, were noticed, and not the ſimultaneous differences at different heights; beſides

* This I have added.

beſides the heats below appeared to have been influenced by reflection, and the heat above by contact with the wall. Thus on the 16th of June, 1784, the heat below being 68°, the heat above was no more than 60°; ſo great a diminution at ſo ſmall a height could not ariſe from the natural progreſſion of heat, but muſt have ariſen from the range of a N E. wind which prevailed on part of that day from which the garden was more ſheltered.

Section II.

Of the Temperature of the Winter Months.

By winter months I underſtand thoſe of November, December, January and February. The temperature of theſe months has preſented ſome extraordinary phænomena hitherto deemed inexplicable, or at leaſt not fully accounted for, though they appear to me connected with and dependant on a fact ſuggeſted by that eminent philoſopher and mathematician Dr. Halley, ſo long ago as the latter end of the ſeventeenth century.

The firſt phænomenon is, that during the winter months, the temperature of the higher ſtrata of the atmoſphere is often warmer than that of the lower.

Thus the temperature of the ſummit of Arthur's ſeat, near Edinburgh, though only 684 feet above the baſe of Hawk-hill obſervatory, was, on the 31ſt of January, 1776, found to be 6° warmer than the temperature below. Phil. Tranſ. 1777, p. 777 and 728. At about eleven o'clock in the forenoon, M. De Luc tells us, that in winter he ſometimes found the temperature on the ſummits of mountains from 2500 to 3000 feet high, exactly the ſame as that of the plains. Modif. § 203. A circumſtance that never occurs in ſummer. Count Fraula, in the 3d volume of the Memoirs of Bruſſels, has ſhewn by experiments, that thaws begin above and are gradually propagated downwards; Meſſier, in the Memoirs of Paris. 1776, p. 19, having placed two thermometers, one at 20 feet above the ground and another 54 feet higher, obſerved this latter to ſtand conſtantly ſome degrees higher than the former, and in one inſtance ſix degrees, on the firſt day of February, 1776, though the weather was ſerene and the wind at eaſt, when theſe obſervations were taken. Ibid. p. 16. The cold below has been attributed to the froſt that ſtill remained unthawed; but granting that its influence could reach to the height of 20 feet, the queſtion is, whence proceeded the change in the temperature of the upper atmoſphere, which ſome ſhort time before was much colder? It evidently did not proceed from the earth, as it conſtantly does in ſummer.

THE ſecond remarkable phænomenon is that the North Pacific Ocean, above latitude 40°, is much colder than the North Atlantic, betwixt the ſame parallels. The interior parts of Siberia, eaſt of longitude 100, are much colder than the parts equally diſtant from that meridian on the weſtern ſide. The coaſt and interior of the weſtern regions of America are much colder above latitude 40° than the correſponding tracts of the European continent.

A THIRD ſingular circumſtance is, that barometers, in the northern parts of Europe at leaſt, generally ſtand higheſt in the months of December, January and February. This has been obſerved almoſt conſtantly at Peterſburgh during ten years, (IX Coment. Petrop. p. 325) that is from 1726 to 1736, and during eleven years at Abo, that is from 1750 to 1761. 25 Schwed. Abhand. p. 112; and by Muſchenbr. in Holland, in the year 1728. La Cotte alſo obſerves, that the higher and loweſt ſtates of the barometer occur in the winter months. 44 Roz. Jour. p. 232. It is alſo well known that the ſmalleſt variations occur within the tropics, but gradually increaſe as we recede from them. Ibid.

Now the fact ſtated by Halley, and with which all the above phænomena appear to me to be connected, is, that the equatorial air,

air, and that of the tropic to which the ſun approaches, " being " rarefied by heat and preſſed upon by the colder air, riſes and " diffuſes itſelf above, forming a current in a contrary direction " to the ſubjacent inferior current of the colder air; ſo that a NE. " wind below is attended with a SW. wind above, and a SE. " below with a NW. above." Here his ſtatement ends, but the laſt part of it is erroneous, or at leaſt ambiguous; for from it one would be apt to deduce the exiſtence of two ſimultaneous ſuperior currents, one on the northern and the other on the ſouthern ſide of the Equator, whereas he moſt probably meant two ſucceſſive currents, as he tells us that it is the air of the tropic to which the ſun approaches that flows in a contrary direction to the colder air below; now as the ſun cannot at the ſame time approach both tropics, it follows that theſe currents, like the ſun's approaches, muſt be ſucceſſive; ſo that when the ſun is in, or approaches to the ſouthern tropic, that is, during the winter of the northern hemiſphere, a SE. current prevails in the upper regions of our hemiſphere, and when the ſun, during our ſummer, approaches to, or is in the northern tropic, a NE. wind prevails in the upper regions of the ſouthern hemiſphere; and in fact this equatorial intumeſcence muſt neceſſarily flow and diffuſe itſelf in that direction in which it meets with leaſt reſiſtance, and it meets with leaſt reſiſtance in blowing towards that hemiſphere in which at equal heights the air is moſt rarefied; now when the ſun is in,

or

or approaches to the northern tropic, winter reigns in the ſouthern hemiſphere, therefore the lower ſections of the atmoſpheric columns are more condenſed by cold, and conſequently the upper ſections of thoſe columns are proportionably rarer (ſuppoſing the abſolute weight the ſame) than the correſponding ſections of the northern hemiſphere, where, from the great expanſion occaſioned by the heat below, a greater part of their maſs reaches to the ſame height as the more rarefied part of the ſouthern columns. Therefore during our ſummer, or the winter of the ſouthern hemiſphere, the greater part of the intratropical intumeſcence flows to the *ſouth*. Halley adds alſo to the *eaſt*, in order to preſerve the equilibrium; but this ſeems a miſtake, the equilibrium is ſupported by the inceſſant circumvolving flow below. The direction of the ſuperior current is guided only by the greater or leſſer reſiſtance it meets with; it muſt move, as he himſelf ſays, "*from* thoſe "parts where the greateſt heat is," and conſequently towards the colder, which at that height muſt be the rarer, and offer leaſt reſiſtance; now the weſtern parts, over which the ſun's influence has not as yet been exerted, are evidently colder than the eaſtern, over which the ſun has already paſſed, therefore the ſuperior current is directed weſtward, or in other words a NE. wind prevails above. The reverſe takes place in the northern hemiſphere during our winter, or in other words a SE. wind prevails in the upper regions of our atmoſphere.

Again,

Again, another difference muſt be remarked betwixt the direction of the trade winds below and that of the ſuperior current. The trade winds are chiefly eaſterly, with only a few points to the north or ſouth, according to their ſituation on the north or ſouth ſide of the equator, commonly one or two, rarely more. But the direction of the upper current is chiefly to the north or ſouth, according to the tropic the ſun approaches, with only a few points weſtwards, as the greateſt cold prevails in the northern or ſouthern quarters.

The height above the level of the ſea or ſurface of the earth, at which this intumeſcence begins to overflow, is that at which its denſity notably ſurpaſſes that of the aggregate of the adjacent extratropical columns at the ſame height, that is about $\frac{1}{35}$ or $\frac{1}{36}$th, and even ſtill leſs.

To ſtate this point more clearly we muſt take a general ſurvey of the temperatures of the different aggregates of air thus compared, confining ourſelves to the northern hemiſphere, as beſt known, and to the *winter* ſeaſon.

The

The equator and tropics lie chiefly over ſea, but partly over land, and the temperatures of each of theſe are very different.

Mean heat of the equatorial air -	84°
——— of the ſupra-marine S. intra-tropical air	85°
——— of ditto incumbent over land -	98°
——— ſupra-marine N. intra-tropical air	80°
——— of ditto incumbent over land -	90°
At the level of the ſea and ſurface of land. mean of all	87°

Mean heat of the extra-tropical ſupra-marine air in this ſeaſon*, from latitude 23° to latitude 33°	66°
Ditto of that incumbent over land -	70°
Mean of both at the level of the ſea and ſurface of the land - - -	68?

To repreſent the action of theſe on each other, we ſhall ſuppoſe each to form a diſtinct column, and both columns to be contiguous to each other, and each to ſupport mercury in the barometer to the height of 30 inches, at the ſurface of the earth.

* See Eſtimate of the temperatures of different latitudes.

And here it is plain in the firſt place, that, as both are of equal weight, the intra-tropical column, being more expanded by heat, muſt reach to a greater height than the colder and leſs expanded extra-tropical air. But that at a certain height the weight and denſity of the intra-tropical air muſt be conſiderably greater, and conſequently that this air muſt flow over or into the other, will now appear by ſhewing the elevation that mercury would ſtand at in each, at that given height. The height I now allude to is that of the upper line of congelation; in the mean of the above-mentioned latitudes of the *extra-tropical* air, this height is 21800 feet = 3633 fathoms, which, ſubtracted from log. 30 (= 4771212) gives the logarithmic number 1138212; and this correſponds with the natural number 12,997, I. to this height, the mercury would then riſe in the barometer. But in the column repreſenting the *intra-tropical* air, the mercury would riſe at the ſame elevation over the earth to 13,835 inches, as appears by the following calculation. The mean height of the line of congelation of the intra-tropical air is 25000 feet, and the mean heat of this air, at the ſurface of the earth being 87°, the difference of this with 32° is 55°, which divided by 250,00 quotes 0,222 the common difference of the progreſſion; and this multiplied into 21800 (218,00 being expreſſed in the terms of the progreſſion) gives the diminution of heat

heat 48°,396, which, ſubtracted from 87° gives the temperature at that height, 38,664. The mean heat of this column is therefore 62°,8, which exceeds the temperature of 32° by 30°,8. Now, according to Sir George Schuckburgh's calculation, 1000 feet of air, by one degree of heat above 32°, gain 2,43 feet, conſequently 21800 feet of air ſhould gain 52,97 feet. And if one degree of heat gives an increaſe of 52,97 feet, 30,8 degrees ſhould give 1631,476 feet; conſequently the mercury ſhould riſe in it to the ſame elevation as if the barometer had been placed 1631,476 lower, that is at the height of 21800 — 1631,476 = 20168,524 feet = 3361,420 fathom, which ſubtracted from log. 30, as above, gives the logarithmic number 1409792, correſponding with the natural number 13,835. At this number of inches then the mercury would ſtand in the intr-atropical air. The denſity of the ſuperior air incumbent upon it exceeds that of the air incumbent over the extra-tropical air of the ſame height over the earth by ,838 of an inch of mercury, or about $\frac{1}{36}$ of the whole weight of each column. It muſt therefore flow over, or into the rarer extra-tropical air.

This overflow takes place at even far lower heights in the atmoſphere; for by a ſimilar calculation it will be found that the denſity of the intra-tropical air exceeds that of the extra-tropical by $\frac{1}{10}$ of an inch, even at the height of 8000 feet.

This ſuperior current is inceſſantly propelled forward by the inceſſant ſucceſſion and propulſion from behind; and its rapidity increaſed proportionally to the decreaſed denſity of the more northern columns to which it proceeds.

The heat it poſſeſſes at the height of 21800 feet was given at the average both of the ſupra-marine and ſupra-terrene columns; but it is evident that the heat of the ſupra-marine columns at that height is ſome degrees lower, and that of the ſupra-terrene ſome degrees higher. At the height of 1000 feet the heat of the ſupra-marine intra-tropical air is 57°,6, and of the ſupra-terrene 70°,6.

Sir Charles Blagden, in a very intereſting paper inſerted in the Philoſophical Tranſ. 1781, p. 341, has ſhewn that the Gulf ſtream, paſſing northwards from the Gulf of Mexico through water ſeveral degrees colder, loſes only two degrees of heat for every three degrees of latitude it paſſes through; and Dr. Franklin, in a paper of four years later date, in the ſecond volume of the American Tranſ. p. 316, informs us that this ſtream preſerves its ſuperiority of temperature at leaſt from latitude 25° to latitude 44°, which it reaches in twenty or thirty days, that is through 19 degrees of latitude about 1300 miles; but Sir Benjamin Thompſon (now Count Rumford) in the Phil. Tranſ. of 1786, p. 304, has ſhewn that atmoſpheric air is four times a worſe conductor of heat than water

water is, and conſequently parts with it more ſlowly in that proportion. Therefore it loſes only 2° of heat in paſſing through 12° of latitude, and 6° degrees in paſſing through 24° of latitude, and 8° in paſſing through 48°. Therefore the *ſupra-terrene* intra-tropical air at the height of 10000 feet is cooled down in latitude 72°, that is 48° beyond the northern tropic, only 8°, and conſequently its temperature, even in that diſtant latitude, is 64°. But the *ſupra-marine* intra-tropical air, whoſe temperature at the height of 10000 feet is 57°, is cooled down to 49° at latitude 72°. It is true, that the degree of moiſture, in the air through which the intratropical air paſſes, muſt occaſion a conſiderable difference as to the heat it retains, for Count Rumford has alſo ſhewn, that moiſt air is 3,9 times a better conductor of heat than common air, and conſequently nearly as good a conductor as water. Ibid. But the air, in his experiments, was ſaturated with moiſture, a circumſtance that cannot be ſuppoſed in the caſes at preſent referred to. From theſe principles the explication of the above-mentioned phænomena is clearly deduced

For 1°. the ſuperior ſtrata of the atmoſphere are obviouſly warmer than the lower, being occupied by the ſuperior current, whoſe heat is gradually communicated to the lower ſtrata, until at length it becomes uniform, as in the caſes obſerved by De Luc.

2do.

2d°. The North Pacific Ocean is colder than the North Atlantic between the ſame parallels, becauſe the ſuperior current that paſſes over the North Pacific is entirely *ſupra-marine*, and for the ſame reaſon eaſtern Siberia is much colder than its more weſtern tracts. But the current that paſſes over the eaſtern parts of the North Atlantic is in great meaſure *ſupra-terrene*, for it iſſues from Guinea, Senegambia, and a ſkirt of the great deſert. So alſo the current that paſſes over the weſtern parts of Siberia, being derived from Siam, Ava, and the more ſouthern iſlands. But the current that paſſes over the eaſtern regions of North America is entirely *ſupra-marine*, as it originates on the Atlantic Ocean; whereas that which paſſes over the correſponding European tracts, Hungary, Poland, Germany, Sweden, France, Spain, Italy, and the Britiſh iſlands, is entirely *ſupra-terrene*, ariſing from air ſuper-incumbent on ſouthern and northern Africa. Theſe directions from the ſouth to the N. N. Weſt may eaſily be traced on a map, obſerving to allow from one to three points to the weſtern direction.

The 3d phœnomenon is due to the re-inforcement of the ſame cauſe. In the months of December, January, and February, the ſuperior current is then more copious, as the intra-tropical air is is then more heated, and hence adds more to the weight of the northern air, and conſequently mercury in barometers muſt ſtand higheſt; but as this current ſoon diffuſes itſelf over regions

on

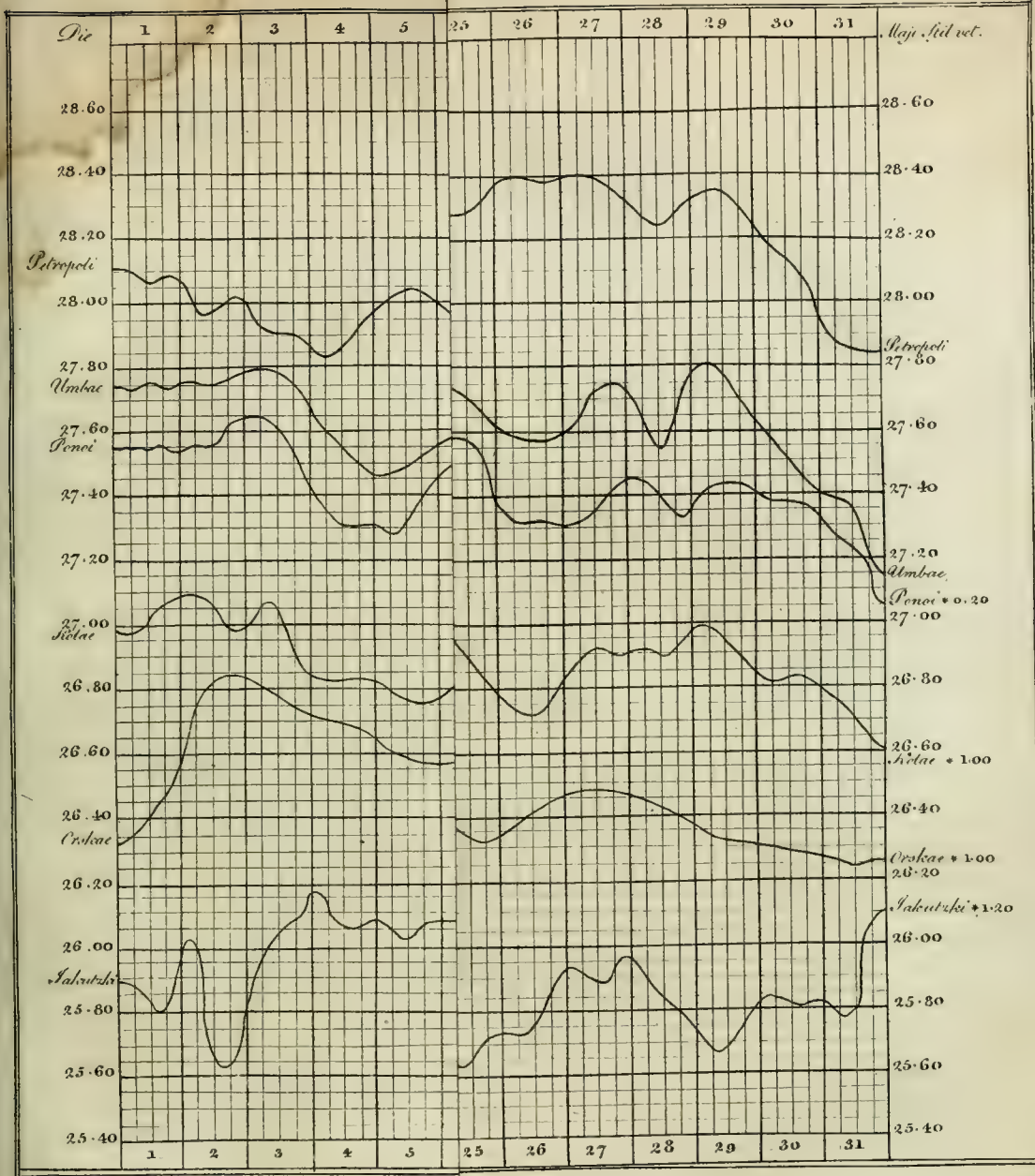
on which air at an equal height is ſtill more rarefied, the mercury muſt ſink in proportion to the quantity of air that deſerts its ſtation. The ſmall variations of mercury in the intra-tropical regions proceed from the ſmall alteration of the quantity of air incumbent over thoſe tracts. The perturbations that take place within the tropics originate in far higher ſtrata of the atmoſphere, than thoſe do that take place in the more diſtant extra-tropical tracts. Thus, Gentil has ſhewn, that thunder proceeded from from clouds 10000 feet above the ſurface of the earth at Pondicherry, latitude 12°. 2 Gentil, p. 79. But in latitude 46°, Sir George Schuckburgh heard thunder grumbling under him when ſtanding on mount Saleve, an elevation of only 2831 feet over the ſurface of the plains. Phil. Tranſ. 1777, p. 527. Now as a great part of the weight of the atmoſphere reſides in the inferior and denſer ſtrata, it is evident that it muſt be more altered by the perturbations that happen in them, than by thoſe that happen in the much loftier. Hurricanes alone affect the lower ſtrata, and hence the barometer ſinks conſiderably. Thus, in the hurricane that happened in the iſland of St. Bartholemew on the 2d of Auguſt, 1792, the barometer fell from 30,18 to 28,03 during its continuance, and perhaps ſtill lower, for the obſerver was obliged to quit the houſe, whoſe proſtration he apprehended, when at its height. See the circumſtantial deſcription in XI. Voights's Phy. Magaz. 4 Stuck. p. 74.

It may be objected, that mercury riſes to great heights above its natural ſtate even in ſummer, when no ſuperior tropical effluence prevails, which is certainly true; but it muſt be remarked, that a vaſt acceſſion of air ariſes in that ſeaſon from the decompoſition of water in growing vegetables, as Dr. Hales has abundantly proved, and is now univerſally acknowledged. Nay, Melander Hielm informs us, that the quantity of air emitted from gun-powder during a battle raiſes mercury in barometers ſituated in the vicinity. 5 Nov. Schwed. Abhandl. 1784, p. 9. Nor ſhould we ſuſpect that the atmoſphere ſhould thus receive an annual increaſe, as much is deſtroyed by volcanos, tornadoes, hurricanes, and putrefying ſubſtances, &c.

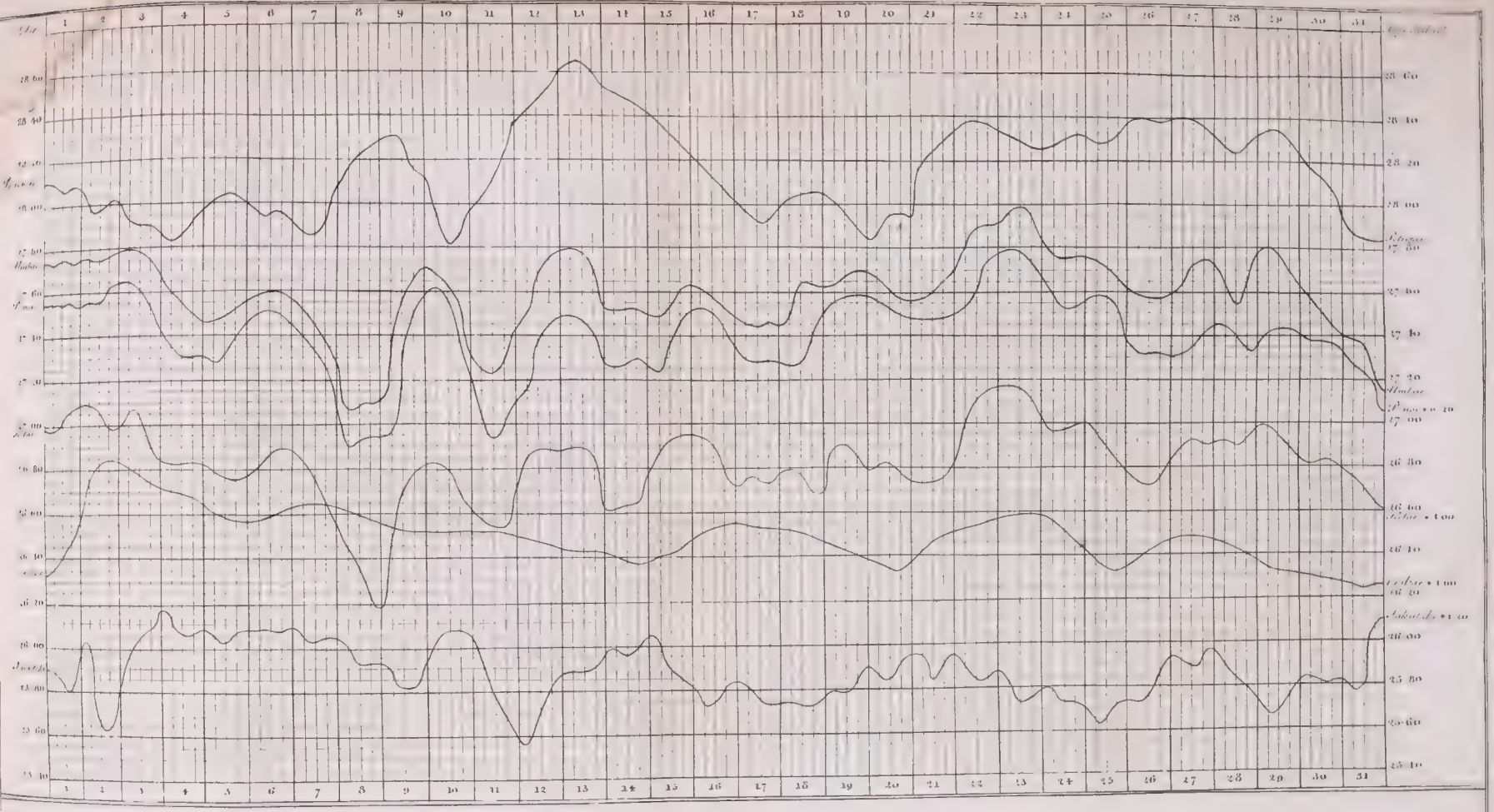
Most of the particulars I have advanced, relative to the influence and difference of the ſuperior currents, receive the fulleſt confirmation from Euler's ſynoptical table of the variations of the barometer during the month of May, 1769, in the vaſt empire of Ruſſia, including an extent of obſervation, amounting to nearly 4000 miles, from the weſtern parts of Ruſſian Lapland to Iakutz in Siberia, and eleven degrees from north to ſouth. The places where the cotemporaneous ſtates of the barometer were obſerved were*:

* This table I have here copied from the 14th vol. of the N. Acta Petropol.

Latitude.



Altitudines Barometriae menc Tabula per lineas curvas repraesentantur: quarum abscisae temporibus respondent. Ne autem hae curvae flexibus earum tortuosis nimium nocentur; et quo metri Penoi, Kotae Orskae et Jakutzki Observatas ita ut pro oppido Penoi 20 partes addendae sint ad Altitudines Barometricas hic notatas.



Explicatio Figurae

Altitudines Barometricae mense Maji Anni 1769 Petropoli Kietae Penci Umbae Orskae et Jakutzki Observatae in hac Tabula per lineas curvas repraesentantur quarum abscissae temporibus Observationum ad Meridianum Petropolitanum reductis et applicatae Altitudinibus Barometri ipsis respondent. Ne autem hae curvae floribus earum bastasses nimium nectantur et quo melius ea a se invicem distingui queant, minores aliquantum redditi vel depressi altitudines Barometri Penci Kietae Orskae et Jakutzki Observatas da ut pro oppido Penci 20 partes centesimae pro Kiela et Orske vero 40 quasvis partes et pro Jakutzk adeo 60 partes centesimae pollicis parisini addendae sunt ad Altitudines Barometricas hic notatas.

	Latitude.	Longitude.
Kola - -	68°,52′ - -	30°
Ponoi - -	67° - -	35°
Umba - -	66° - -	37°
Peterſburgh -	59°,56′ - -	30°
Iakutz - -	62° - -	102°
Orſka - -	51° - -	60°

On examining this table it will be ſeen, 1°. That the mercury riſes *ſooner* in the *ſouthern latitudes* than in the *northern*, (when both are ſubject to the ſame current) thus it was riſing on the *firſt* of May at Orſka while it was falling at Peterſburgh. See alſo May 7th, 15th, and 20th. And it began to riſe ſooner at Peterſburgh than at Ponoi or Umba. See May 4th, 8th, and 11th. But the variations of the riſe and fall are leſs diſtant in point of time, becauſe, though Kola is more northern than Peterſburgh, it is alſo more eaſtern, and therefore the appulſe of the current is more nearly ſimultaneous than its appulſe to Orſka, which is both more ſouthern and more eaſtern, and therefore receives the ſuperior effluence ſooner.

2°. Ponoi and Umba are near each other, and therefore their variations, in every ſenſe, differ little from each other; but Umba being ſome degrees more eaſterly, the riſe there ſomewhat precedes that at Ponoi.

gtio. The variations at Iakutz, proceeding from a different efluence, namely, the *ſubra-marine*, have no connexion with the preceding; this the table ſufficiently indicates in every inſtance, but in none ſo remarkably as on the 31ſt of May, for while the mercury in all the other above-mentioned places is on the fall, (which proves the identity of the influence to which they are ſubjected) it riſes at Iakutz.

This ſynoptical view of barometrical obſervations at different diſtant places is the only one, I believe, as yet exhibited; yet if it had been extended to latitudes ſtill more ſouthern, as it eaſily might, much light could not fail of being thrown on this obſcure ſubject.

Section III.

Of the Origin of the General Trade Winds.

Though the origin of the general trade winds appears to me to have been fully eſtabliſhed by Dr. Halley, yet it ſeems he has explained himſelf too briefly, ſince his explanation has been miſunderſtood by many, and was thought obſcure even by D'Alembert*.

* Sur la Cauſe des Vents. V.

To

To underſtand it more perfectly, let us ſuppoſe the ſun for the firſt time in the meridian, and to communicate its heat every inſtant fifteen degrees all around. If it were to remain in this ſituation the ſurrounding air could have no other motion but upwards, for the lateral dilatations being equal, would neceſſarily check each other, but in the ſecond, and all the ſucceeding inſtants, the ſun moves weſtwards; therefore, of the originally equidiſtant eaſtern and weſtern points, the weſtern, to which the ſun approaches nearer, is more heated than the eaſtern, from which the ſun recedes; therefore, in this, and all the ſucceeding inſtants, the eaſtern, being more cooled, will preſs on the weſtern, and thus an eaſtern wind will be eſtabliſhed.

It is true, that, in the northern hemiſphere, the northern air alſo preſſes upon the more heated ſpaces, but as this alſo follows the ſun's path to the weſtward, it becomes alſo eaſterly, preſerving only a few points of its primitive direction. D'Alembert adds alſo the ſolar attraction, which, according to him, elevates the air in the points over which the ſun is vertical, and, conſequently, produces a dilatation advancing from eaſt to weſt. But Mr. De la Place, not denying this cauſe, conſiders it too weak to produce ſingly any conſiderable effect*.

* Mem. Paris, 1776.

About the year 1735 Mr. Hadley publiſhed a very different account of the origin of the trade winds. 8 Phil. Tranſ. Abrid. p. 500, which, however, has been rejected by the moſt diſtinguiſhed aſtronomers that have ſince attended to this object, as D'Alembert, ſur la Cauſe des Vents. Art. 376, and 385, Gentil Voy. 2. Bergman Erde Beſchreib. p. 91.

According to Mr. Hadley, the air, being rarefied towards the equator, is conſequently invaded, in the northern hemiſphere by the northern, and in the ſouthern hemiſphere by the ſouthern colder air.

But as the parallels of latitude enlarge as they approach the equator, and as the equatorial ſpace is nearly in the proportion of 1000 to 917, the difference of their circumference is nearly 2083 miles; conſequently, the ſurface of the globe at the equator moves ſo much faſter than under the tropics; and hence the northern or ſouthern air, moving from the tropics towards the equator, muſt poſſeſs leſs velocity than the parts it arrives at, and, conſequently, appear to move in a direction contrary to that of the earth's motion, which being from weſt to eaſt, the air arriving ſooner at the weſtern parts, will appear to move from eaſt to weſt, and this relative motion being combined with that towards the equator, a north-eaſt wind will be produced on the north ſide, and a ſouth-

eaſt

eaſt wind on the ſouth ſide of the equator.—Theſe, as they approach the equator, ſhould become ſtronger and more eaſterly, and appear due eaſt in the equator itſelf, by reaſon of the concourſe of both currents from the north and from the ſouth.—There the velocity of each ſhould be at the rate of 2083 miles in the ſpace of one natural day, or above 1.33 miles per minute, if it had not been that before the air at the tropics could arrive at the equator, it muſt have gained ſome motion eaſtwards from the ſurface of the earth or ſea, whereby the relative motion is diminiſhed to the degree that actually exiſts in it.

This theory appears to me rather ingenious than ſolid, for the following reaſons:

1°. The trade winds are commonly gentle, moving only at the rate of eight miles an hour; therefore they have ſufficient time to gain or participate of the motion of the earth; therefore their contrary courſe muſt ariſe from an abſolute cauſe, and cannot be deemed merely relative.

2°. Because the north-eaſt wind ſcarce ever approaches nearer than eight or ten degrees to the equator, and there dies away; whereas it ought there, according to this theory, to be ſtrongeſt. And, on the contrary, the ſouth-eaſt paſſes the equator ſeveral degrees,

grees even when the ſun is in the ſouth tropic. A fact which as Gentil remarks is abſolutely irrreconcilable with this theory. 1 Gentil Voy. p. 638, 5 Gentil p. 116.

3tio. Because if the conſtant eaſterly wind was in the northern hemiſphere ſupplied ſolely from the north, and in the ſouthern hemiſphere ſolely from the ſouth, we ſhould in the former have a conſtant north wind at leaſt at 35 or 40 degrees from the equator, or at leaſt from ſome northern point, and in the latter a conſtant ſouth wind, or at leaſt from ſome ſouthern point; whereas on the contrary a ſouth wind often prevails in thoſe latitudes on the north ſide of the equator, and a north wind on the ſouth ſide.—Thus La Peyrouſe met an E S.E. in north latitude 32°, and a due eaſt in latitude 31°, and a S. S. E. in latitude 14°, and a due eaſt in latitude 16°, and a due north in latitude 20°, (where then was the relative motion?) and a due ſouth in latitude 33°. See his Journal in 3d La Peyr. Voy. He alſo met with a due north in latitude 27° and 42° ſouth, and a N. N. E. in latitude 25° ſouth. So Captain Cook met a S. S. E. wind in latitude 30° north, and alſo in latitudes 40° and 41°, and a due ſouth wind in latitude 38° and 20°; and in the ſouthern hemiſphere a due north in latitude 3°, 4°, and 44°. I might produce other inſtances from ſea journals, and particularly from that moſt ample and inſtructive, kept by Major Dalrymple during a voyage to the Eaſt Indies. Phil. Tranſ. 1778, but

but I think the alleged ſufficiently prove that the general eaſt wind is not ſupplied ſolely from the north or ſouth in the different hemiſpheres reſpectively.

4°. Because during our ſix ſummer months, when the ſun is in or approaches to the northern tropic, the eaſterly trade wind partakes leſs of the northerly, than when the ſun is in or approaches to the ſouthern tropic. 2d. Phil. Tranſ. Abrid. p. 134, and Schued. Abhandl. 1762, p. 175, which is directly contrary to Hadley's ſyſtem; for when the ſun is in the ſouthern tropic, the north wind muſt traverſe more of that ſpace in which the earth's motion eaſtward is ſtrongeſt, and therefore ſhould participate more of that motion as Hadley himſelf ſtates: though ſtill partaking of it in a ſmaller degree than that which the globe itſelf poſſeſſes, it ſhould appear to move weſtwards; yet it ſhould proportionably retain leſs of its original direction from north to ſouth, than when it had traverſed a ſpace more diſtant from the equator, whereas the fact is, that it retains more, and often paſſes into the ſouthern hemisphere into the 13° ſouth latitude without having any eaſtern direction. 3 Marchand, p 551; and an analogous fact is obſerved with reſpect to the ſouth eaſt wind when the ſun is in the northern tropic. Hence it is evident, that it is from the approach of the ſun, and not from the latitude traverſed, that the eaſtern direction is derived; nay the wind is often more eaſterly than northerly between latitude 23°

and

and 28, Foſter's Obſervations p. 126. He even obſerved, that the trade winds extended far beyond the tropics when the ſun is in the ſame hemiſphere, which ſhews it is the ſun that cauſes them.

Eddy is a term inttoduced on this ſubject, which explains nothing when its cauſe is not aſſigned and proved; the trade winds are often interrupted by the approach of land, but the interruption, as Foſter mentions, extends only to a few miles. Ibid. 127.

The *monſoons* or periodical trade winds, depending on local circumſtances ſufficiently explained by Dr. Halley, I ſhall here paſs over; though certainly much may be added from obſervations made by ſubſequent navigators and travellers. I ſhall therefore confine myſelf to the *variable* winds, a ſubject much more obſcure.

Section IV.

Of Variable Winds.

With reſpect to winds we muſt lay down one general and fundamental principle, which is, that they always originate at the extremity of that point towards which they proceed. Thus the

the eaſterly trade wind begins at the point neareſt the ſun, which it follows, and is perpetually renovated and ſupplied from parts ſtill more eaſterly.—Thus in the year 1709 a north wind was ſooner perceived in England than at Dantzick. 4 Phil. Tranſ. Abrid. 2d part, p. 115. And Wargentin notes that when the wind changes to the weſt, this change takes place at Moſcow before it happens at Abo, which is ſeveral degrees weſt of it, and ſooner in Finland than in Sweden. Schwd. Abhandl. 1762, p. 195. And Dr. Franklin, in his XXXVI. Letter, p. 389, thinks that the north-eaſt ſtorms in North America begin firſt in point of time in the S. W. parts; that is to ſay, ſooner in Georgia than in Carolina, and ſooner in Carolina than in Virginia, &c. He found that a north-eaſt ſtorm began at Philadelphia at ſeven o'clock, but did not extend to Boſton (about forty miles to the north-eaſt) until eleven o'clock. The reaſon of which he well explains, as the current muſt begin in the places neareſt to that in which the rarefaction ariſes towards which the current is directed.

Of Weſterly Winds.

That eminent and laborious meteorologiſt, citizen La Cotte, infers from numerous obſervations of many years, that between

latitude 47° and 60° on the weſtern ſide of our hemiſphere, the weſt wind, with ſome participation of the north or ſouth, is that which obtains oftenеſt. 39 Roz. Jour. p. 267. Leche obtained the ſame reſult at Abo, latitude 60°, from twelve years obſervations, Muſchenbroock in Utrecht, Mr. Dalton in Weſtmorland, latitude 54° (ſee his Meteorolog. Eſſays, p. 48 and 88) from five years obſervations.

This wind in our continent originates in the Pacific ocean, between the above mentioned parallels, at leaſt in winter; the air incumbent on that ocean is then much warmer than that of Siberia and Chineſe Tartary that lie weſt of it, this therefore preſſes upon and flows into the ſupra-marine, and is immediately ſucceeded by air ſtill farther weſtwards, and thus a current is gradually eſtabliſhed extending to the Atlantic, which, though in winter, being much warmer than the air of the iſlands and continent on which it flows, is forced into the current, both by the rupture of the equilibrium to the eaſtwards, and by the preſſure of the much colder air of the continent of North America.

Of

Of Eaſterly Winds.

During the winter months there ſeems to be a frequent ſtruggle and conteſt betwixt the air incumbent over the Aſiatic continent, and that incumbent on the North American, lying betwixt the above-mentioned parallels and bordering on the Pacific ocean, which of them ſhall rule over it.

The maſs of the American air being leſs conſiderable, and its efforts divided between the Pacific and the Atlantic, is generally obliged to yield to its antagoniſt; though ſometimes the Aſiatic being warmed, either by a diffuſion of the ſuperior current or by ſoutherly winds, the colder American becomes more forcible. In ſummer this muſt happen frequently, the E. N. E. oftenеſt prevailing; upon the whole however Leche remarked that the E. and E. S. E. were nearly the moſt uncommon; as did La Cotte in the climate of Paris. Meteorolog. p. 305.

With us this wind is moſt frequent in the months of April and May; and I have obſerved in Cook's Journal, tables 9th, 10th and 11th, that it prevails alſo in the ſame months in the Pacific, therefore the colder continental air then pours in upon us.

La Cotte alſo obſerves, that in the weſtern tracts of Europe, in latitudes below 48°, this wind occurs oftenеſt during the winter months*, for the ſuperior heat of the Atlantic in the low latitudes determines the colder air incumbent on Hungary and European Turkey to flow in upon it.

Of Southerly Winds.

A few years ago no problem in meteorology appeared to me more difficult than to aſſign a cauſe for the frequent prevalence of a ſouth wind even in winter, it being contrary to the laws of nanature that warm air ſhould ruſh upon colder, yet I ſince diſcovered, that the conjectural ſolution I then offered is grounded on a real fact.

In the eaſtern parts of our hemiſphere, from longitude 72° to 160°, that is, from the coaſt of Malabar to the Moluccas, it blows from the north-eaſt conſtantly from October until April. Now this northern blaſt muſt be ſupplied and recruited from countries ſtill farther north, until we arrive at the pole, and the polar air muſt conſequently be ſupplied by that which lies ſouth of it, and

* 2 Mem. Meteorolog. p. 189, &c.

thus

thus a ſouthern current is eſtabliſhed on the weſtern ſide of our hemiſphere.

Instances to ſupport or contradict this theory do not often occur, yet I have found ſome that appear to me deciſive, independently of the general reaſon alleged. Thus I find in the 9th table of the third vol. of Cook's Voy. that in north latitude 59°, and eaſt longitude 207, on the 25th of May, 1778, a ſtrong north-weſt wind prevailed; and on the 29th day of the ſame month and and year, an equally ſtrong S. W. wind prevailed at Peterſburgh, latitude 60°, and longitude 30 E. Now the places of obſervation were 177 degrees diſtant, one on the eaſtern and the other on the weſtern ſide of our hemiſphere, (which, at this proximity to the pole, argues not, a ſuperior diſtance to that I have mentioned) and four days is as ſhort a time as can be allowed to the S. W. to ſupply the more eaſtern N. W. Mem. Peterſburgh, 1778, p. 92. So alſo in the ſame journal I find, that from the 4th to the 30th of May, a north wind prevailed in the eaſtern part of our hemiſphere from latitude 58° to 61°, except ſeventeen days of variable winds; but in London it blew from the S. W. during the firſt fifteen days of June, thus replacing the northern air. And to replace the conſtant N. E. wind, on the Indian Peninſula to the Moluccas, there is a conſtant draught from the ſouth in the weſtern parts of our hemiſphere; accordingly Leſke obſerved, that on an average of

of twelve years, it blew 126 days each year, from October until May, from ſome ſouth point, namely, 86 days from the ſouth or ſouthweſt, and 40 from the ſouth-eaſt, at Abo, lat. 60°.

It is true, that he found it to take place very frequently alſo in ſummer, but this is occaſioned by the great heat that then prevails in the northern tracts of Lapland.

And, upon the whole, more of the ſouth air is drawn off in winter than in ſummer, for its flow is gentle in ſummer, but often ſtormy in winter. See Leſke's IX. XI. and XII. tables. If all other meteorological tables of a ſeries of years had been arranged with equal ſagacity and preciſion as thoſe of Leche and Dr. Horſeley, a vaſt fund of information might be extracted from them.

At Peterſburgh, during the year 1793, Euler junior found a ſouth or ſouth-weſt wind prevailed 79 days, 52 from October to the end of March, and only 27 in the ſummer months; it was ſtormy in November, December, and January. I have not noted the S. E.

Mr. Stritter alſo found the ſouth wind to predominate at Moſcow during the ſix winter months of that year. 11 N. Acta Petrop. p. 569. So that the frequency of this wind in high latitudes is certain.

Of

Of Northerly Winds.

In the weſtern parts of our continent and hemiſphere theſe are of all others the leaſt frequent in latitudes above 48°. See La Cotte and Leche's tables. The cauſe of this unfrequency appears from what has been ſaid of ſouth winds.

But in latitudes below 48° they occur oftener, and oftenеſt in thoſe that are ſtill lower, as La Cotte remarks. An admirable inſtance of Divine Providence, that the warmeſt winds ſhould prevail oftenеſt in winter in the coldeſt regions, and cold winds in the warmeſt!

But it may be aſked, why a ſouth wind ſhould not prevail in the eaſtern parts of our hemiſphere to ſupply the conſtant N. E. wind that prevails in the low latitudes of the weſtern ſide? The reaſon is, that on the weſtern ſide the N. E. winds of low latitudes are eaſily ſupplied by the contiguous Atlantic, which is open up to the North Pole; and, as here, the upper current ſets and ceaſes, there can be no deficiency of air.

Of

Of oppoſite Concomitant Winds.

It has often been obſerved*, but of late, ſince the invention of baloons, evidently proved, that currents of air from different and even oppoſite points of the horizon, prevail at different heights in the atmoſphere, over the ſame tracts of land or water. This was originally inferred from the different courſes of the higher and lower clouds; but as ſuch obſervations were often liable to optical deceptions, better proofs were wanting.

Mont Louis is within thirty miles of Perpignan, but about 5000 feet higher. Now in March 1780 north and north-eaſt winds prevailed at Perpignan and a weſterly wind at St. Louis. In Auguſt a north wind prevailed at Perpignan and an eaſt at Mont Louis. Mem. De La Société de Medecine de Paris, 1780. Derham ſuſpected†, and Gentil has ſince ſhewn, that changes of ſeaſons conſtantly begin in the upper atmoſphere; while a ſtrong wind blows from one point below, a wind from an oppoſite point reigns above, but more gentle, until at laſt (in about three weeks) it is propagated downwards. 2 Voy. p. 23 and 24, in 8vo. The lower atmoſphere, he ſays, extends to the height of 2880 feet.

4 vol.

* 2 Ulloa's Voy. p. 62, Engliſh.

† 4 Phil. Tranſ. Abridg. 2 Part. 125.

4 vol. p. 48. At the commencement of winter, when the ſun approaches the ſouth tropic, and the north air begins to flow in and follow it, it muſt meet with more reſiſtance from the lower denſer air, as its impetuous courſe in an oppoſite direction is more ſlowly altered (this reſpects the monſoons) than in the rarer ſuperior ſtrata; and the ſame effect, but in a different direction, takes place when the ſun approaches to the northern tropic.

It has been ſaid by many, that winds in the ſuperior regions of the atmoſphere are much more violent and impetuous than in the lower. Sauſſure Hygrom. p. 300. 2 Ulloa's Voy. p. 81. Muſchenbr. § 2612. 2 Bergm. Erde kugel. p. 99. De Luc, &c. But the contrary has alſo been obſerved by Gentil, above quoted, and Morveau. Aeroſt. de Dijon.

Of the Succeſſion of Winds.

Well eſtabliſhed general laws on this head would be extremely uſeful, as we might then foreſee what wind might next be expected. Beſides the *general* ſucceſſion in an open country, it is probable there is a *local*, confined to certain ſituations.

Gentil remarks, that in the ſouthern latitudes of our hemiſphere, a north-eaſt is ſucceeded by an eaſt, ſouth-eaſt, and ſouth.

According to La Cotte, the order of ſucceſſion in the middle latitudes is ſouth-weſt, north, weſt, north-eaſt, ſouth, north-weſt, eaſt, ſouth-eaſt. 39 Roz. Journ. p. 267.

Of the Scirocco.

This is a ſouth, or ſouth-eaſt wind, known in the ſouthern parts of Italy, Sicily and Malta, diſtinguiſhed by peculiar debilitating effects, well deſcribed by Brydone, and by Dolomieu in his treatiſe on the temperature of Malta. The latter has ſhewn that its malignity reſults from the conſtitution of the air it conveys, and not merely from its temperature, which is variable, from 55° to 80°. It contains a much ſmaller proportion of oxygen than air uſually does. The conſtitution of the African wind, called Harmattan, is as yet unknown; it is, at leaſt on land, loaded with ſome unknown undiſſolved vapour, and is much hotter and drier than the Scirocco, but not debilitating, and even wholeſome for animals; for though it parches their ſkin it deſtroys infection and cures ſeveral diſorders. See Phil. Tranſ. 1781, p. 46, &c. Its direction is alſo weſtwards.

Section VI.

Of the Variations of the Temperature of the Summer and Winter Seaſons that take place in different Years.

To reaſon with preciſion on this ſubject, we muſt at firſt abſtract from all ſublunary phyſical cauſes, and indicate the temperature appropriated to different latitudes from mere aſtronomical conſiderations.

Halley has ingeniouſly reſolved this problem ſo far as the mere ratios of heat in the different ſeaſons are concerned. 2 Phil. Tranſ. Abr, p. 165. And Lambert in his Pyrometric, § 596.

Halley, calculating the *ratios* of heat communicated by the ſun to the earth, (which he conſiders merely as a planet, abſtracting from all diſtinction of land and water) in the different ſeaſons in the northern hemiſphere, reduces theſe ſeaſons to three, the equinoxes, the ſummer ſolſtice, and the winter ſolſtice; and attending only to the ſines of incidence of the ſun's rays, and the duration of their action, he ſets the heat communicated at latitude 0. On the days of the vernal and autumnal equinoxes at 20000. And on the tropical days in the ſame latitude at 18341. And then adds the ratios which the heat in every 10th degree of north latitude bears to theſe at the ſame periods. Lambert adds the ratios of lat. 49° and 66°,33′, ſtating the equatorial heat on the equinoxial day at 999.

But to expreſs theſe ratios in thermometrical meaſures we muſt endeavour to find the greateſt heat of the equinoxial day, taking a mean of the heat of the morning at two o'clock, and the evening, under the equator, or very near it; and this I find to be 88° or 89° of Fahr. (See Ulloa, Mem. Philoſoph. p. 61) in the northern hemiſphere, on the 20th of March, on the ocean, to which indeed we muſt confine ourſelves in this enquiry, and particularly the Atlantic, for no uniformity can be expected on land.

It is uncertain what thermometer of Reaumur Ulloa employed, whether the true or the falſe, and hence I place the heat at 88? of Fahr.

This correſpondence being found, the thermometrical degrees correſponding with all the other ratios are eaſily found by the rule of proportion, and the degrees thus found I call the *mathematical temperature*. Bat in moſt caſes this temperature is far from agreeing with the temperature really obſerved; and which I therefore call the *real* temperature; this I take at a *mean*, and not at its *maximum*, which I could not always diſcover, and is more fugitive and contingent. Theſe temperatures I exhibit in two ſeparate tables, the firſt indicating thoſe of the vernal equinox and of the northern tropic or midſummer, and the ſecond thoſe of the autumnal equinox and the ſouthern tropic or midwinter, over the Atlantic or ſtandard ocean in our hemiſphere.

Table

Table the Firſt.

Vernal Equinox.			Midſummer.	
Latitude.	Mathem.	Real Mean.	Mathem.	Real Mean.
0	88°	84°	80,72	83°
10	86,5	82	89,3	84,3
20	82,69	77	95,64	80,5
30	76,21	69,5	99,66	73,5
40	67,4	60	101,41	70,5
49	61	51	101,7	62 Lambert.
50	56,5	50,5	101,86	61
60	44	40	100,2	56
66°,33′	39,81	34	101,21	55 Lambert.
70	30,99	32	101,41	54
80	15,28	27	108,56	51
90	9,6 Lambert.	. . .	109,93	. . . .

In this table we ſee, 1°. That during the vernal equinox the heat differs but little from the *mean* heat really obſerved in all latitudes, and perhaps ſtill leſs from the *maximum* of real heat. Yet, except in latitude 80°, it is always higher, both from the quantity of rays loſt in paſſing through the air, and from the quantity reflected by water and the frequent interpoſition of clouds, &c.

2dly. We ſee that the aſtronomical heat conſtantly increaſes with the height of the latitudes, as the duration of the ſolar rays more than compenſates for their obliquity, when the ſun is in the northern tropic; but the *real heat* decreaſes as the latitudes increaſe, becauſe this theoretic compenſation does not take place, from the interpoſition of clouds and the acceſs of cooler winds, and the increaſed reflection from the ſurface of the water.

The different temperatures of different ſummers are ultimately reſolvable into the different direction of the winds during thoſe ſeaſons, and the different electrical ſtates of the atmoſphere, the ſouth or ſouth-eaſt producing not only clouds which intercept the ſun's rays, but alſo copious rains or hail, which, deſcending from great heights and occaſioning a copious evaporation, cool the air to a great degree. The north and north-eaſt, on the contrary, unleſs immediately ſucceeding great rains, (for then they increaſe the

the evaporation) diſperſe the clouds, and proceeding from countries then ſomewhat heated, allow the ſun's rays their natural calefactive effect. But why winds from oppoſite points ſhould prevail in different years, cannot be known, until the contemporaneous ſtates of the atmoſphere between the northern tropic and the equator, are known. It is poſſible that frequent hurricanes and tornadoes, during which a quantity of air may be deſtroyed and converted into water, may demand an annual ſupply from the north, and thus occaſion our north and eaſt winds; and the abſence of theſe phænomena may occaſion an influx from the ſouth, if the north and eaſt are ſummoned to a different quarter, by ſimilar cauſes.

Table

Table the Second.

Autumnal Equinox.			Midwinter.	
Latitude.	Mathemat.	Real Mean.	Mathemat.	Real Mean.
0°	88°	84°	80,°7	83°
10	86,5	84,6	69,66	78,5
20	82,69	80	58	72,5
30	76,21	73,5	44,54	64,5
40	67,41	70,5	30,55	54
49	61,	59	18,23	45 Lambert.
50	56,56	58,5	16,71	44
60	44	48	4,73	34
66,33	39,81	42	1,32	30 Lambert.
70	30,99	39	0, . .	27
80	15,28	33,5	. . .	22
90	9,6	. . .	. . .	. . . .

On

On this table we may remark, 1o. That though the mathematical temperature of the autumnal equinox be exactly the ſame as that of the vernal, yet the real is much higher, of the hemiſphere being cooled during the winter is ſlowly heated, and being heated during the ſummer is ſlowly cooled.

2do. That in conſequence of this circumſtance, the real temperature of the autumnal equinox approaches much nearer to the aſtronomical than does that of the vernal, until we arrive at latitude 70° and the higher latitudes.

3tio. In all latitudes above the equator a cold approaching to the aſtronomical is ſcarce ever felt at ſea in winter; to what can this be attributed but to the equatorial effluence? For other cauſes, viz. evaporation, and the frequent intervention of clouds, or at leaſt haze, intercept the ſun's rays, and conſequently ſhould cool the air even below the aſtronomical ratio which ſuppoſes the incidence of all the rays; in latitudes above 20° the difference is enormous.

4to. At the diſtance of ſome hundred miles from the coaſts of the Atlantic, in latitudes above 40°, the cold is much more moderate than the mathematical ratios indicate, in moſt years; owing to the above-mentioned cauſe, and to the reign of weſterly and

ſouthерly winds, which convey their heat to a conſiderable diſtance before they are cooled.

But in latitudes between 55° and 36° a degree of cold far ſuperior to the aſtronomical has often been obſerved, and particularly of late, and in countries not very diſtant, or even bordering on the Atlantic. Theſe extraordinary ſeaſons may be attributed partly to the abſence of the ſuperior effluence or its refrigeration in communicating with air replete with vapours, and partly to the prevalence of eaſt north-eaſt winds which proceed from the interior and coldeſt parts of our continent, and hence the cold of the year 1776, ſo well deſcribed by Van Swinden, ſeems rather to have followed the order of the longitudes than of the latitudes. Wargentin, ecretary to the Royal Academy of Stockholm, informs him that the cold obſerved that winter in Stockholm was not at all extraordinary, and expreſſes his ſurpriſe that it ſhould have been ſo rigorous in Germany, France and England. Mem. Paris, 1776, p. 129. Nay it appears that the north-eaſt wind which raged ſo furiouſly in Holland and at Montmorenci, latitude 49°, on the 27th (See Van Swinden, p. 40) had not been at Peterſburgh on the 18th, nor any day after; for a perfect calm reigned on that day, and the high winds of the remainder of the month were from the north-weſt. Act. Petropol. p. 382. It is therefore probable that this wind proceeded obliquely from the eaſtern

eaſtern and ſouthern parts of Ruſſia, and may have been derived from latitude 55° and longitude 40°, and originated on the Atlantic, lat. 40°, and hence the utmoſt rigour of this cold was ſooner perceived in the ſouth of France, as Tholouſe, Marſeilles, &c. than in the more northern latitudes, as may be ſeen in Van-Swinden's general table, for it reached theſe towns on the 18th or 19th of January. In the more northern latitudes it was felt only on the 27th; it is true its date at St. Jean de Luz, latitude 43°, is January 28th in the general table, but this is a miſtake, as may be ſeen, p. 181, for January 19th is there ſaid to be the true date, and, p. 179, it is ſaid that the 18th or 19th of January are the days on which the greateſt cold was obſerved in all places ſouth of the Garonne; which fully confirms my former ſtatement that the wind which produced this cold originated in the ſouth-weſt, and thence was gradually propagated northwards and eaſtwards. All the minuter modifications of this cold, in places not very diſtant from each other, may be aſcribed either to recent falls of ſnow, the proximity to which muſt effect more or leſs the thermometers, the greater or leſſer abundance of vapours in the atmoſphere, and other circumſtances too tedious and minute for inſertion in this general view.

Snow falling from ſome height in the atmoſphere is generally for ſome time ſurrounded with an atmoſphere much colder than

the air ſome feet above it, as Mr. Wilſon obſerved, though it did not occur to him that the cold was communicated to the air by the ſnow, for he thought it highly remarkable that a thermometer hung 24 feet above the ſnow was four degrees leſs cold than one ſuſpended 2½ feet above it. Phil. Tranſ. 1780, p. 462. Yet Mr. Boyle has long ſince noticed a ſimilar fact, (1 Boyle Abridg. p. 629) as related to him by ſome navigators, and Foſter expreſsly mentions, that being to the leeward of an icy mountain (probably many feet diſtant) the thermometer ſunk four degrees, and roſe to its former height when he had paſſed that mountain, (Obſervat. p. 73) but when there is not a recent fall of ſnow, the air ſeveral feet above the ſurface of the earth is generally colder (when no great evaporation takes place) than that nearer to its ſurface. Thus during the intenſe cold of January 1776, there having been no fall of ſnow ſince the 24th, Van Swinden found the degree of cold on the morning of the 27th to be 8°,25, while Camper, in the ſame ſtreet, whoſe thermometer was ſome feet nearer to the earth, found it only 6°,5. See Van Swinden, ſur le froid de L'Annee 1776, p. 24, 25, 28 and 176.

Section VII.

Of the Temperature of the Southern Hemiſphere.

That the ſouthern hemiſphere is colder than the northern is the general opinion. 1 Gentil Voy. p. 114. That it is ſo, during its ſummer ſeaſon, can ſcarce be denied, at leaſt in latitudes above 40°.

But the winter of this hemiſphere ſeems not to be as cold as ours between the ſame parallels (ſee Eſtimate of the temperature of different latitudes, p. 51) the higher latitudes are rarely navigated during their winter.

Œpinus, reaſoning on aſtronomical principles, attributes the inferior temperature of the ſouthern hemiſphere to the ſhorter abode of the ſun in the ſouthern tropic, ſhorter by ſeven days, which produces a difference of fourteen days in favour of the northern hemiſphere, during which more heat is accumulated; and hence he infers that the temperature of the northern hemiſphere is to that of the ſouthern as 189,5 to 175,5 or as 14 to 13. Mem. Peterſb. 1761.

On the other hand, Mr. *Prevoſt*, of Geneva, and of the Academy of Berlin, grounded on phyſical conſiderations, namely, that the ſouthern winds incroach three degrees on the northern ſide of the equator, and that the north trade wind reaches no farther than to the fifth degree, even on the north ſide of the equator, infers that the temperatures of the north and ſouth ſegments, taken on theſe limits, are to each other inverſely as the ſegments, and hence calculates the temperature of the north ſegment to be to that of the ſouthern as 11 to 9. 38 Roz. Jour. p. 369. He would have ſtated this ratio * ſtill higher, if he had obſerved, as *Moore* does in his late excellent Treatiſe on Navigation, that the northern trade wind extends only to the tenth degree

* Some, I know, object that no ratio of heat can be juſt, becauſe its firſt original degree cannot be found by obſervation; but of what quantity can the abſolute unit be rigorouſly found? Muſt we therefore diſcard the units of weights and meaſure? The higheſt degree of cold muſt vary in various ſubſtances like fuſibility, for it muſt be that, beyond which the attractive power of their particles ceaſes, the reſiſtence to a nearer approach being infinite; now the force of this power muſt and does vary in various bodies, and therefore the point or degree at which it reaches itt *maximum* muſt vary alſo; with reſpect to air, I have reaſon to think it does not exceed 335 degrees below the freezing point. With reſpect to other bodies, 72° beneath the freezing point may to moſt practical purpoſes be aſſumed as the *maximum* of cold, being the degree at which mercury freezes.

degree of the north latitude, more or leſs according to the ſun's declination, p. 126.

VARIOUS obſervations tend to juſtify both theſe ratios, when applied to different latitudes, but neither of them apply within certain other latitudes.

THE following table exhibits, as far as I can collect, the mean heat of the correſponding latitudes of each hemiſphere at ſea, during the ſummer months of each, extracted as to the ſouthern hemiſphere, from the journals of Cooke, La Peyrouſe, Dalrymple and Marchand; in low latitudes the winter months alſo.

SOUTHERN.

Latitude.	Degrees.	SOUTHERN.	NORTHERN. Corresponding Months.
		December.	*June.*
From Lat.	0°	From the 9th to the 15th, inclusively	I cannot exactly state the mean of the first 10 degrees during this month, but from latitude 10° to 15°-
to	15°	Mean - 83°,57	
		3d Cook's Voy. Table 6th.	the mean is - 84°,
		From the 7th to the 20th - 78°,5	Mean - - - 83°,5
	14°	La Peyrouse and in Cook - 85°,	This agrees nearly with *Epinus's* determination.
		Mean - 81°,7	
		April.	*October.*
		From the 9th to the 23d, inclusively	
	18°	Mean - 81°,18	Mean of the Month - 80°,
		3d Cook's Voy.	
		July.	*January.*
	22°	From the 18th to the 27th, inclusive-	Mean heat of an equal number of
	23	ly, 4 days in lat. 22°, 2 days in	days in similar latitudes, north 67°,
	24	lat. 24°, 2 days in lat. 25°, and	
	25	2 in lat. 26°.	Hence the southern winter is warmer
	and		than the northern.
	26	Mean heat - 72°,5	
		3d Cook's Voy.	

March

Latitude.	Degrees.	SOUTHERN.	NORTHERN. Corresponding Months.
		March and April.	*September and October.*
From Lat.	0°	Five last days of March and six first days of April, mean — 71°,	Mean heat of an equal number of days — — — — 75°,
to	27°	La Peyrouse's Voy.	
		March and April.	*September and October.*
	34°	15th March — — 69°	Mean of both months equal — 69°
		3d April — — 76	Of September — — 72°,5
		10th do — — 69	
		Cook's 4th and 19th table, mean 71	
		But would certainly be less if the whole of April were comprehended.	
		1st of March (Marchand) — 72°,5	
		May. Cape of Good Hope.	*November.*
		Per Sparman — — 58°	Mean heat — — 62°,
		June.	*December.*
		Per Sparman, mildest winter 57°	Mean — — — 60°,

Latitude.	Degrees.	SOUTHERN.		NORTHERN. Corresponding Months.	
		Auguſt.		*February.*	
From Lat.	0°	Mean (per Dalrymple) -	62°,7	Mean - -	63°,
to	34°				
		November, December, January and February.		*May, June, July and Auguſt.*	
		Per Stavorinus's account of the mean of the aggregate of the four months - -	74°,5	Mean of the aggregate at ſea	72°,5
				That of land is higher.	
		December the 3d. (Cook's)	64,		
		January 21ſt. (La Peyrouſe)	72,5		
		March.		*September.*	
	39°	From the 5th to the 11th, per Cook - - -	67°75	Mean - - -	71°,
		But the remainder of the month probably colder.			
		January.		*July.*	
	43°	From the 19th to the 31ſt, mean	59°,44	Mean of the ſame days -	65°,
		Hotteſt day -	67,5		
		Coldeſt - -	56		
		Cook.			

December

Latitude.	Degrees.	SOUTHERN.	NORTHERN. Corresponding Months.
		December.	*June.*
From Lat.	0°	From the 9th to the 22d (La Peyrouse), mean - - 50°,4	Mean - - 67°,
to	44°	And in lat. 42° and 43°, the hottest day did not exceed - 55,6	
		December.	*June.*
	48°	From the 15th to the 29th, mean 43°,	Mean - - 63°,
		Janudry.	*July.*
		From the 1st to the 5th and from the 10th to the 15th, inclusive, mean - - - 47°, La Peyrouse.	Mean - - - 65°,
		Janury and February.	*July and August.*
	58°	From the 29th Jan. to the 8th February, mean - 43°,7 La Peyrouse.	Mean - - - 56°,5

Hence we see that up to latitude 40° the temperature of the northern is to that of the southern nearly as 14 to 13,5, and from latitude 40° to latitude 50° as 11 to 9, and from latitude 50° to 60° as 8 to 6 nearly, in the summer season.

CHAP. IV.

Of the Denſity of the Atmoſphere.

Denſity is the relation which the quantity of matter in any ſubſtance bears to the bulk of that ſubſtance. It might as well have been ſaid, that it is the relation that the bulk of any ſubſtance bears to the quantity of matter contained in it, had it not been that the quantity of matter remains unaltered while the bulk varies. As the *quantity* of matter in all ponderable ſubſtances is proportioned to the *weight* and is no otherwiſe diſcoverable, the weight is often ſubſtituted for the quantity of matter. Hence the *maſs* of any body is equal to its denſity multiplied into its bulk.

To compare with preciſion the bulk and quantity of matter in any ſubſtance, it is neceſſary that a given weight and a given bulk be aſſigned, in which the relation betwixt them is aſcertained and expreſſed in known weights and meaſures, which may ſerve as a *ſtandard* in all other caſes.

The bulk of all elaſtic fluids is affected and varied both by a variation of *preſſion* and a variation of *temperature*; to aſſign therefore a general ſtandard for the denſity of theſe fluids, it is neceſſary to diſcover

diſcover ſome general circumſtance in which a known preſſure, a known temperature, and a known weight may be aſcertained as nearly as phyſical reſearches admit or demand. Now this known and general circumſtance is the temperature of the congelation of water, or 32° on Fahr. thermometer, or 0 of Reaumur, and is invariable all over the globe. The known *preſſion* is that of 30 inches Engliſh of mercury, whoſe ſpecific gravity in temperature 60° or 62° is 13,6 nearly, on a baſis of one ſquare inch Engliſh meaſure; a preſſion which may be univerſally aſcertained, as mercury may be eaſily purified ſo as to poſſeſs that ſpecific gravity, at leaſt very nearly, and minute preciſion is in moſt caſes unneceſſary, at leaſt in meteorology. And, laſtly, the known *meaſure* is 87 feet, or 1044 inches, or 14,5 Engliſh fathom, this being the quantity of air which in this circumſtance weighs as much as one-tenth of a cubic inch of mercury, that is, 344,32 grains at the level of the ſea, at which the general mean height of the barometer is 30 inches.

This determination poſſeſſes this peculiar advantage, that if the preſſion *varies*, the temperature 32° remaining unaltered, (as when the barometer ſtands above or below 30 inches) the meaſure or number of fathoms of air that poſſeſs this weight may be diſcovered by the table of common logarithms, rejecting the characteriſtic; for if the barometer ſtands at any given height, then ſubſtracting from the four firſt figures on the left of its logarithm, the four firſt figures

figures of the log. of a number one-tenth lower, the remainder thus found gives fathoms, and the reſidue, decimal parts of fathoms.

Fathoms multiplied into 6 give feet, and multiplied into 72 give inches. The fathoms, feet, and inches are paralelipipeds, which, for the purpoſe of finding their weight, I ſuppoſe to ſtand on a baſis of one ſquare inch; conſequently the inches are cubic inches, of which 1728 make one *cubic foot*, as do 144 of theſe altitudinal paralelipipeds, feet; and ſo do 24 of theſe paralelipiped fathoms. All this is commonly known, but is proper to mention here for greater clearneſs. That at the temperature of 32° the difference of the logarithms gives the height of air in Engliſh meaſure, has been ſhewn by General Roy. Sir George Schuckburg indeed ſuppoſes this to happen at the temperature of 31°,24, but the difference is of no conſequence.

Though this ſubject is perfectly elementary, yet ſome examples and the ſolution of a few problems will be uſeful to thoſe that are little accuſtomed to it. In theſe I all along ſuppoſe the temperature conſtant, namely, at 32°.

Examples.

Examples.

BAROMETER being at 30 inches, what is the length of a vertical column of air that weighs as much as the tenth of a cubic inch of mercury?

HERE 29,9 is the number which is $\frac{1}{10}$ lower than 30. Then log. 30—log. 29,9 gives the length required, in fathoms and parts of fathoms.

Log. 30 = 477121
Log. 29,9 = 475671

Then the length required is - 14,50 fathoms.

HERE 14, is the difference of the four firſt figures of the logarithm, and 50, that of the two laſt figures, and therefore decimal parts.

Note.—When the barometrical height or heights are marked by mixed numbers, their logarithms muſt be ſought as if they were integral numbers, and if one of them be an integer, it muſt be raiſed by the addition of a cypher or cyphers to the ſame number of places as the other has.

THUS in the above example 29,9 is conſidered as 299, and 30 as 300.

Second.

Second.

BAROMETER being 29,5, what is the length of a vertical column of air that weighs as much as $\frac{1}{10}$ of an inch of mercury?

Here the number next lower is 29,4.

Then log. 295 = 469822
log. 294 = 468347

14,75 fathoms.

HERE the preſſure being diminiſhed the length is greater than in the former Example.

Third.

BAROMETER being 29,54, what is the length of a vertical column of air that weighs as much as $\frac{1}{10}$ of an inch of mercury?

Here the number 29,54—0,1 = 29,44. Then log. 2954=470410
log. 2944=468938

Length = 14,72 faths.

Fourth.

Fourth.

BAROMETER 30,05, what is the length of a vertical column of air that weighs as much as $\frac{1}{10}$ of an inch of mercury?

Here 30,05—0,1=29,95. Then log. 3005=477844
Log. 2995=476397
Length = 14,47 fathoms.

Fifth.

BAROMETER 28,77, (which is its mean height at Geneva) what is the length of a vertical column of air that poſſeſſes the above-mentioned weight?

Here 28,77 — 0,1 = 28,67. Then log. 28,77 = 458940
Log. 2867 = 457428
Length = 15,12 fathom.

HENCE they breath, at a mean, lighter air than we do in Dublin, in the ratio of 1412 to 1450, at the temperature of 328, and alſo drier.

Sixth.

BAROMETER 30, what is the length of a vertical column of air that weighs as much as $\frac{1}{20}$ of an inch of mercury = 172,16 grains?

$-\frac{1}{20}$=0,05 and 30—,05=29,95. Then log. 3000=477121
Log. 2995=476397

Length = 7,24 fathom,

which is nearly the half of the weight of $\frac{1}{10}$ of an inch of mercury. In this manner, the length correſponding, to any other proportional part of an inch of mercury may be found. And reciprocally, the weight being given, the length of the vertical column poſſeſſing that weight may be found, firſt ſeeing what proportional part of the weight of an inch of mercury, viz. 3443,2, the given weight makes.

Seventh.

BAROMETER 29,5, what is the length of a vertical column of air that weighs as much as one inch of mercury?

Here 29,5 — 1, = 28,5. Then log. 295 = 469822
Log. 285 = 454845

Length = 149,77 fathom.

Eighth.

Eighth.

BAROMETER 30, what is the weight of a cubic foot of air in this temperature (32°)?

WE have already ſeen in the firſt example, that 14,5 fathom of air weigh 344,32 grains, and alſo premiſed that one cubic foot = 24 fathom. Now as 14,5. fathom 344,32 grains :: 24. fathom 569,99 grains (nearly 570 grains). Barometer 29,5 a cubic foot of air weighs but 560,249 grains. See ſecond Example.

Note.—The weight of a cubic foot of air in this temperature being thus found barometer 30, the weight of a cubic foot of air at any other barometrical height *below* 30 *inches* (=B) may be found though not exactly, yet very nearly, the difference not exceeding half a grain, for as 30. B :: 569,99.x. Thus as 30.29,5 :: 569,99 to 560,49, which differs only by 0,241 from the truth.

AND if the barometrical height given *exceed* 30 inches, then as B.30 :: x. 569,99.

So alſo 144 altidudinal feet on a baſis of one ſquare inch, weigh, barometer 30, 569,99 grains; ſo alſo do 1728 cubic inches,

from which the weight at any other barometrical height may in the ſame manner be deduced. Thus barometer 30 in temperature 32°, 1000 cubic inches of air weigh 329,855 grains; then if the barometer be at 29,5, then 1000 cubic inches of air will weigh by this rule 324,357 grains; for as 30. 29,5 :: 329,855. 324,357, but more exactly 324, 218; for ſince a cubic foot of air at barometer 29,5 weighs 560,249 grains; then as 1728. 560,249 :: 1000. 324,218; the difference however is inconſiderable.

To expedite calculations of the weight of air, I have formed the annexed table of the weight of 1000 cubic inches of air (equal 13,888 fathoms or 83,333 feet,) at the following barometrical heights.

Weight

Weight of 1000 *Cubic Inches of Air at Temperature* 32°.

Barometer.	Grains.	Barometer.	Grains.	Borometer.	Grains.
31,—	340,84	30,3	322,1	28,6	303,4
,9	339,74	,2	321,0	,5	302,3
,8	338,6	,1	319,9	,4	301,2
,7	337,5	29,—	318,8	,3	300,1
,6	336,4	,9	317,7	,2	299,0
,5	335,3	,8	316,6	,1	297,9
,4	334,2	,7	315,5	27,—	296,8
,3	333,1	,6	314,4	,9	295,7
,2	332,0	,5	313,3	,8	294,6
,1	330,9	,4	312,2	,7	293,5
30,—	329,85*	,3	311,1	,6	292,4
,9	328,7	,2	310,0	,5	291,3
,8	327,6	,1	308,9	,4	290,2
,7	326,5	28,—	307,86	,3	289,1
,6	325,4	,9	306,76	,2	288,0
,5	324,3	,8	305,66	,1	286,9
,4	323,2	,7	304,5	26,—	285,8

* More exactly 329,810.

Barometer.	Grains.	Barometer.	Grains.	Barometer.	Grains.
26,9	284,7	24,—	263,8	23,1	242,9
,8	283,6	,9	262,7	22,—	241,8
,7	282,5	,8	261,6	,9	240,7
,6	281,4	,7	260,5	,8	239,6
,5	280,3	,6	259,4	,7	238,5
,4	279,2	,5	258,3	,6	237,4
,3	278,1	,4	257,2	,5	236,3
,2	277,0	,3	256,1	,4	235,2
,1	275,9	,2	255,0	,3	234,1
25,—	274,8	,1	253,9	,2	233,0
,9	273,7	23,—	252,8	,1	231,9
,8	272,6	,9	251,7	21,—	230,8
,7	271,5	,8	250,6	,9	229,7
,6	270,4	,7	249,5	,8	228,6
,5	269,3	,6	248,4	,7	227,5
,4	268,2	,5	247,3	,6	226,4
,3	267,1	,4	246,2	,5	225,3
,2	266,0	,3	245,1	,4	224,2
,1	264,9	,2	244,0	,3	223,1

Barometer.	Grains.	Barometer.	Grains.	Barometer.	Grains.
21,2	222,0	20,4	213,2	19,6	205,4
,1	220,9	,3	212,1	,5	204,3
20,—	219,8	,2	211,0	,4	203,2
,9	218,7	,1	210,9	,3	202,1
,8	217,6	19,—	209,8	,2	201,0
,7	216,5	,9	208,7	,1	200,9
,6	215,4	,8	207,6	18,—	199,8
,5	214,3	,7	206,5		

And in general the weight of 1000 inches, barometer 30 being given, the weight at any proportional part of 30 may be found, it being the ſame proportional part of the weight at 30.

Thus the weight at barometer 30 is 329,855 then 20 being $\frac{2}{3}$ of 30 the weight at barometer 20 is $\frac{2}{3}$ of 329,855 grains=219,8 grains as in the table. The weight at barometer 10 (being $\frac{1}{3}$ of 30) is 109,7 grains, the weight at barometer 1 is 10,99, the weight at barometer 0,1 is 1, 099 grains.

The weight of 1000 inches ſtanding on a baſis of one ſquare inch, barometer 30 is 329,855 grains.

Note.

Note.—Alſo that 1000 inches = 83,3333 feet ſtanding on the ſame baſis, and conſequently poſſeſs the ſame weight, as do alſo 13,8888 fathom.

Problem, I.

To find the height that mercury would ſtand at, in a barometer at a given elevation in the atmoſphere, the height at which it ſtands at the bottom of that elevation being alſo given, and the mean temperature of the air being alſo 32°.

Solution.

If the height be given in feet or inches, convert them into fathoms or parts of a fathom. Subtract theſe fathoms (or parts of a fathom) from the log. of the height of the lower barometer, the remainder will give a logarithm belonging to the natural number, denoting the inches at which mercury ſhould ſtand in a barometer on that elevation.

Example.

Barometer below being at 30 inches, at what height ſhould mercury ſtand, on an elevation of 1000 feet? Mean temperature 32°.

Here

HERE 10000 feet = 1666,66 fathom. And log. 30 = 477121. And 4771,21 — 1666,66 = 3104,55.

Now in Sherwin's table I find log. 31045 correſponds with the natural number 20,439 at this height, therefore the mercury in the upper barometer will ſtand.

PROBLEM, II.

THE height of mercury in a barometer on a certain elevation being given, to find the height of the mercury in a barometer at the bottom of that elevation. Mean temperature, 32.

Solution.

CONVERT the feet or inches expreſſing the elevation into fathoms, or parts of a fathom. Add theſe fathoms, &c. to the four firſt figures of the log. of the number of inches at which the mercury ſtands in the upper barometer, the ſum will give the logarithm of the height of the mercury in the lower barometer, which ſeek in the table,

Example.

THE upper barometer ſtanding at 20,439 on an elevation of 10000 feet, at what height does it ſtand in a barometer at the bottom of that elevation. Mean temperature 32°?

Here 10000 feet=1666,66 fathom. And the log. of 20,439 is 310459. Now 310459 + 166666 = 477125 which correſpond with the natural number 30. This then is the mercurial height in the lower barometer.

Corollary.

HENCE an elevation, and the mean temperature of 32° being given, the height at which two barometers would ſtand may be found, if the height of either be alſo given; otherwiſe the problem is undeterminate, and recourſe muſt be had to the general poſition, that the mean height of a barometer ſtanding at the level of the ſea is 30 inches.

But note.—That if the height of the mercury in both barometers be not equal, they muſt be equalized as to that circumſtance, by extracting the exceſs of one over the other, ariſing from the expanſion of mercury by heat. The method I employ of effecting this equation is ſimilar to that deviſed by Dr. Horſely; namely, by ſubſtracting the degrees of heat of the colder barometer, from thoſe of the warmer, and multiplying the difference into 0,40, and ſubtracting this product from the difference of the logarithms.

PROBLEM

Problem, III.

The height at which mercury ſtands in two barometers, one at the ſummit of any elevation, and the other vertically under it, or at leaſt not more than ten miles diſtant from a vertical ſituation (the nearer the better), and the mean temperature being 32°; to find the meaſure of that elevation, and the weight of of the intermediate column of air.

Solution.

Find the logarithms of thoſe mercurial heights. The difference of the four firſt figures on the left of thoſe logarithms gives the meaſure of that elevation in Engliſh fathoms; and the difference of the remaining three figures gives the decimal parts of thoſe fathoms, the whole multiplied into ſix gives feet, or multiplied into 72 gives inches, as already ſaid. The height of the mercury in both barometers ſhould be taken at the ſame inſtant, after allowing them ſufficient time to aſſume the temperature of the air, or at leaſt to cool.

To find the weight of the intermediate column of air, obſerve by how many inches, or parts of an inch, the mercury

in the upper barometer differs from its height in the lower barometer. The weight of the intermediate column of air is juſt equal to the weight of that number of inches, or parts of an inch of mercury. Now a cubic inch of mercury, temperature 32°, weighs 3443,2 grains; therefore if the mercury in the upper barometer has fallen n inches, or parts of an inch, the intermediate column will weigh $3443{,}2\ n$. Thus if the mercury has fallen only $\frac{1}{10}$ of an inch, the weight of the intermediate column of air is $3443{,}2 \times 0{,}1 = 344{,}32$ grains.

Problem, IV.

The elevation being given, and the height of the mercury above and below being found, to find whether the mean temperature of the intermediate column of air were above or below 32° or at 32° exactly.

Solution.

If the number of fathoms in the elevation *exactly corresponds* with the difference of the logarithm of the mercurial heights then the mean temperature of the column was exactly 32°. If the number of fathoms in the elevation *exceeds* the number indicated

indicated by the difference of the logarithms, then the mean temperature was higher than 32°. If it falls ſhort of the number indicated by the difference of the logarithms, then the mean temperature was below 32°.

Section II.

Having found and determined by its weight the denſity of air and that of its variations, as far as theſe may be occaſioned by the natural variations of *preſſion* in the temperature of 32°, it now remains to determine how far under each degree of preſſion it is altered by variations of temperature above and below 32°.

There are few ſubjects on which the opinions of philoſophers differ ſo widely as on this. The principal reaſon of which variance appears to me to have been, that the experiments they appeal to were made in manomeeers of different dimenſions in the bore and of glaſs differently compoſed and differently heated during its fuſion. From this laſt circumſtance its conducting power with reſpect to heat is various; but in all ſorts of glaſs veſſels air adheres to them with more or leſs force, according to their compoſition, in low heats (that

(that is beneath 170°), and moſt in the loweſt, and in thoſe whoſe bore is ſmalleſt; hence much heat is loſt in vanquiſhing this adherence, and, when it riſes or falls, it does ſo by irregular jumps or ſtarts, to ſay nothing of the internal inequalities of the bore, nor of the adherence of the mercury which confines the air.

The perſon who, as appears to me, has moſt ſucceſsfully avoided or leſſened moſt of theſe obſtacles is Mr. Schmidt, Profeſſor at Gieſſen. The deſcription of his apparatus is too prolix for inſertion and cannot be underſtood without a plate. It may be ſeen in 4 Gren's Journal. His experimeuts were made partly on air artificially dried, and partly on air artificially moiſtened, but the degree of moiſture was determined by no known hygrometer. However he found the expanſions of dry air to be exactly proportional to the degrees of heat it was expoſed to, and in this point he agrees with D'Amontons, Lambert and De Luc. Sir George Schuckburg, abating ſome inconſiderable variations, draws the ſame concluſion in all heats from 32° to 83° under a preſſion of 30,5 inches of mercury, and infers from the mean of thirteen experiments that 1000 meaſures of air gain 2,43 by each degree of Fahr. above 32° and gain the ſame increaſe under a preſſure of 40 inches or 23,5 inches of mercury. This laſt point however is contradicted

dicted, and as I think, very reaſonably, by D'Amontons, Lambert, and lately in an excellent memoire of Monge in the 5th vol. of the *Annales de Chemie.*

By the experiments of general Roy, and thoſe of Prieur Duvernois, 1 *Ann. Chemie*, the expanſions of air are various at various degrees of heat. See Philoſ. Tranſactions, 1777. The general alſo aſſerts, that air under a preſſure of 30 inches of mercury, or only of 25 inches, undergoes ſimilar expanſions, p. 708.

Rejecting manometers, as I think they ſhould be rejected, it appears to me that there are three teſts by which the ſuitableneſs of any coefficient ſhould be tried. The firſt is conformity with the weight found at 32°. The ſecond is the weight of the ſame volume of air at different temperatures, found by accurate weighing; and the third is the agreement of barometrical menſuration calculated according to the given coefficient, with the geometrical menſuration.

Note.— The difference betwixt 32° degrees and any degree above or below it I ſhall call in the ſequel the *fundamental difference.*

Examination

Examination of Schuckburg's Coefficient by the firſt Teſt.

Firſt Experiment.

Barometer 29,27 inches (29.3) } 1°. He found a bottle containing
Thermometer 53° } 13558,5 grains of water, and conſequently containing 53,55 cubic inches. As 253,18 grains of water occupy the ſpace of a cubic inch, temperature 62°, to weigh 16,22 grains leſs when exhauſted of, than when full of air. after making every neceſſary allowance. Philoſ. Tranſ. 1777, 560. Conſequently 53,55 cubic inches of air of that temperature, and under that preſſion, weighed 16,22 grains. Now if 53,55 cubic inches weigh 16,22 grains, 1000 cubic inches ſhould weigh 302,89 grains, and ſhould retain that weight under any diminution of volume by cold, as there is no diminution of the quantity of matter.

2do. To reduce theſe 1000 inches of air to the volume they would occupy at 32°, I find that 53° exceeds 32° by 21 degrees; then ſubtracting from 1000 inches the number of inches, which according to Schuckburg they gained by twenty-one degrees above 32, namely $2,43 \times 21 = 51,03$ inches, we have the volume they would occupy at 32°, namely 948,97 (ſay 949) inches. And theſe ſhould weigh 302,89 grains.

3tio.

3tio. Now by the table, 1000 cubic inches at 32°, barometer 29,3 weigh 322,1 grains; and conſequently 949 cubic inches ſhould weigh 305,67 grains. Therefore the error of Sir George Schuckburg's coefficient is exactly 305,67—302,89 = 2,78 grains in defect; therefore his coefficient retrenched too little, and ought to be higher. It is needleſs to examine General Roy's coefficient at this temperature, though it being higher (2,51) it muſt be leſs defective.

The coefficient which agrees beſt with this experiment is 2,84.

The eaſieſt method of finding how many meaſures, 1000 or any other meaſures of air gain by any temperature above 32°, is to multiply 1000, or ſuch other meaſures, into that fundamental difference, and divide the product by 400, that being the reciprocal of the coefficient $\frac{2,5}{1000}$, which is that I employ; as applying more exactly in general than any other, though in ſome caſes others may be more exact.

Second Experiment.

Barometer 28,77 } 1°. According to Sauſſure, hygrometer.
Thermom. 68° } p. 284, a cubic foot of *dry* air weighs 751 grains, barometer 27, thermometer 16°.

Or in Engliſh meaſures, &c. 2090,8 cubic inches weigh 616 grains, barometer 28,77, thermometer 68°, conſequently 1000 cubic inches ſhould weigh 294,62 grains.

2do. The difference between 68° and 32° is 36°. Then by Schuckburg's coefficient 2,44 × 36, ſhould be ſubſtracted from 1000 inches to reduce them to the volume they would occupy at 32°, namely 87,48 inches. Now 1000 — 87,48 = 912,52 inches.

3tio. By the table, 1000 cubic inches at 32°, under the preſſion of 28,7 mercurial inches, weigh 315,5 grains, and conſequently 912,52 inches ſhould weigh 287,90, but the number found by Schuckburg's rule weighs 294,62. Its aberration therefore amounts to 6,72 grains in exceſs. This however may proceed from the circumſtance that this air was perfectly *dry*, and therefore heavier than atmoſpheric air uſually is.

The coefficient for this air, which is far leſs expanſible than moiſt air, ſeems to be 1,29.

Third Experiment.

Barometer 29,84 } According to Lavoiſier (See the French
Thermom. 54.5 } tranſlation of my Eſſay on Phlogiſton, p. 39.)
100 cubic inches of common air weigh 46 grains, barometer 28, thermometer

thermometer 10°, or in equivalent Engliſh denominations 121 cubic inches weigh 37,73 grains, and conſequently 1000 cubic inches weigh 311,81. But this determination not being the reſult of ſingle experiments made in theſe circumſtances, but a mean collected from experiments made at different barometrical heights and in different temperatures, is not ſufficiently rigorous to ſerve as a teſt in this caſe.

Fourth Experiment.

Barometer 30,32
Thermom. 68°.
Hygrom. 75

1o. This is one of ninety-eight experiments made by me with an excellent air pump of Haaſs's in the year 1786, on the weight of air at different barometrical heights from 30,36 to 29,03, and at different temperatures from 73° to 41°. The receiver contained 116 cubic inches very nearly, which weighed 34,75 grains; conſequently 1000 cubic inches ſhould weigh 299,56 grains.

2do. 68° ſurpaſs 32° by 36 degrees, conſequently deducting from 1000 what they gained by 36° of heat (namely by Schuckburg, $2{,}43 \times 36 = 87{,}48$) there remain 912,52, which at temperature 32° weigh 299,56 grains.

3tio. Per table, 1000 cubic inches at 32°, barometer 30,3, weigh 333,1 grains, therefore 912,52 ſhould weigh 303,96 grains. The error of this coefficient is therefore 303,96 — 299,56 = 4,4 grains in defect; it retrenches therefore too little.

I SHALL quote no more of theſe experiments, as, not having made them with a view of ſo nice a teſt as the preſent, I cannot anſwer for their accuracy to half a grain, and the greateſt nicety would be requiſite. Yet I believe, upon the whole, this to be a better method of finding a coefficient than any manometrical experiments.

Method of finding the Coefficient.

1°. THE barometrical height being found to three decimal places, and the temperature from 80° to 33° true to one decimal place, and alſo the hygrometer, find the weight of 1000 cubic inches of air truly to $\frac{1}{10}$ of a grain = *W*.

2do. FIND the weight of 1000 inches at 32° at the ſame barometrical height = *t*, by the table, or ſtill more nicely. Now ſince *t* denotes 1000 inches, *W* alſo at 32° will denote a lower number of inches, ſince it is a lower weight; then ∴ *t*. 1000 :: W. N.

3tio.

3tio. Let the thermometrical degrees above 32° (that is their difference with 32°) = D. and let 1000 N = n, then D x = n, and $x = \frac{n}{D}$. This gives the coefficient ſought.

Example.

Thus in the firſt experiment, barometer 29,3, thermometer 53°. W = 302,89 grains. and t = 322,1 grains. Now ſince 322,1 grains denote 1000 cubic inches, 302,89 at the ſame temperature ſhould denote 940,36 inches = N. and 1000 — 940,36 = 59,64, the number of inches gained by 21° of heat; therefore putting x for the coefficient, 21 x = 59,64, and x = 2,84.

In the ſecond experiment I find, by ſimilar reaſoning, the coefficient to be nearly 2,000, but as this relates to air much drier than any hitherto found in the atmoſphere, it is only uſeful ſo far as to ſhew that the *moiſter* air is the higher, muſt be the coefficient. It varies therefore according to the ſtate of the air as to moiſture between 2. and 3.

In the fourth experiment, hygrometer 75°, I found it 2,77, but with us the hygrometer is generally above 80°. If, therefore, the hygrometrical mean be unknown, I take it that 2,5 is, at leaſt of round numbers, the ſafeſt coefficient.

To

To prove the fitneſs of this coefficient it is neceſſary to give ſome inſtances of barometrical menſuration, wherein it is juſtified by reſults nearly agreeing with geometrical menſuration.

Barometrical menſuration is founded on this principle, that the denſity of the atmoſphere and the height of mercury in a barometer *decreaſe* in a geometrical progreſſion, correſponding with an *increaſing* arithmetical progreſſion as we aſcend into the atmoſphere; though it is only in the mean temperature of 32° that this geometrical progreſſion can be truly indicated in Engliſh meaſures. The method therefore followed is, after equating both barometrical heights ſo far as theſe heights are occaſioned by difference of temperature*.

1°. To deduce from the corrected difference of the logarithms the heights reſulting from the geometrical progreſſion, as ſhewn, p. 439. This is called the *Logarithmic reſult*.

And 2dly, as this height is always inferior to the real height when the mean temperature of the atmoſphere ſurpaſſes 32°, to add to the logarithmic reſult, the quantity which that column of air gains in conſequence of its mean temperature being ſome degrees

* The moſt convenient mode of correction is ſhewn, p. 438.

degrees above 32°, the quantity thus found is called the *calorific reſult* or *ſupplemental* height; the union of both gives the *compleat* height.

HENCE we ſee the neceſſity of having four thermometers, one attached to each barometer to indicate the temperature of the mercury in each, and two detached, one at the top and the other at the bottom, to indicate the temperature of the air; the mean heat of both is ſuppoſed to indicate the mean temperature of the air, and the difference of this mean and 32° muſt alſo be expreſſed. This I call the *fundamental difference.*

3tio. THE indications of the inſtruments being minuted, and the barometers, or rather their logarithms, *corrected*, as ſhewn, p. 438, the logarithmic reſult is obtained by ſubſtracting the logarithm of the barometrical height at the top from that of the barometrical height at the bottom of the elevation, as ſhewn, p. 439. This is alſo called the *approximate height.*

4to. To obtain the *calorific reſult*, or *ſupplemental* height, two products are neceſſary. For, 1°. the fundamental difference muſt be multiplied into 2,5, and divided by 1000; or, more ſhortly, into ,0025, which is the ſame thing. The reaſon is this, 1000 meaſures of air at 32° gain by a heat of one degree above 32°, 2,5 meaſures;

meaſures; therefore by any higher number of degrees above 32° they gain ,0025 multiplied into that number. This is the firſt or *fundamental product.*

2do. This product multiplied into the logarithmic reſult, expreſſed in fathoms, gives the ſecond product, which affords the calorific reſult or ſupplemental height, and which I call the *ſupplemental product.* For ſince 1000 meaſures of air gain by multiplication into the number of degrees expreſſed in the *fundamental* difference, the quantity indicated by the firſt product, it is plain that the meaſures indicated by the logarithmic reſult would gain by multiplication into that product the quantity which reſults from that multiplication. This product therefore added to the logarithmic reſult in temperatures above 32°, or ſubſtracted if below 32°, gives the *compleat* meaſure of the elevation.

A few examples will render this proceſs more intelligible to many. I ſelect them chiefly from Sir George Schuckburg, becauſe he had the rare advantage of having the cotemporaneous indications of his inſtruments at one ſtation noted by intelligent perſons, while he himſelf obſerved them at another ſtation, namely, the higheſt. General Roy had ſometimes the ſame advantage, but I fear not always, and hence probably the defective meaſure of Mount Elio in Wales.

Meaſurement

Measurement of Mount Saleve.

First Series.

Lower Barometer 28,399. Log. 4533030
Upper - - - - 25,712. Log. 4101359

Difference = 431,671
Correction — ,236

Logarithmic Result 431,435 fathoms.

Heat of the Mercury below 78°
- - - - - - - - above 72°,1

5°,9×,4

Correction = 0,236

State of the Atmosphere and Calorific Result.

Heat of the Air above 65,0
- - - - - - below 73,9

Mean 2)138,9(69,4

Fundamental Difference 69°,4
— 32,

= 37,4

First Product.

37,4×,0025

=0,0935

Second Product, or Logarithmic Result, multiplied into the first Product.

431,435 × 0,0935

= 40,334 fathom.

Compleat Height.

431,435
+ 40,334

= 471,769 fathom × 6 = 2830,614 feet.

Trigonometrical height = 2831,3 feet. Error of the barometrical measurement in *minus* 2831,3 — 2830,614 = 0,686 foot.

Third Series of Observations on Saleve. Philos. Tranf. 1777, p. 531.

Lower Barometer 28,393 Log. 4532113
Upper - - - - 25,690 Log. 4097641

Difference = 434,472
Correction — ,56

Logarithmic Result = 433,912 fathom.

Heat of the Mercury below 71°,1
- - - - - - - - above 69°,7

Difference 1,4 × ,4

Correction = ,56

Calorific Reſult.

Heat of the Air, above 62°
- - - - - - below 72,5

2)134,5(67,2

Mean 67,2
— 32,

Fundamental Difference = 35,2

First Product.

35,2 × ,0025

= 0,088

Second Product, or Logarithmic Reſult, multiplied into the firſt Product.

433,912 × 0,088

= 38,184 fathom.

Compleat Height.

433,912
+ 38,184

= 472,096 fathom × 6 = 2832,576 feet.

Error.

2832,576
— 2831,3

= ... 1,276 foot in *plus*.

Sum of the Errors of both Obſervations.

1,276 — ,686 = 0,590 of a foot.

THESE inſtances evidently prove, that the errors of the barometrical calculation of heights proceed ſolely from the difficulty of obtaining the true mean temperature of the atmoſphere, as there are hours in which the aſcent of heat does not follow an arithmetical progreſſion, as Pictet has ſhewn, (ſuppoſing the inſtruments truly read). The propereſt hours are thoſe between ſun-riſe and the hotteſt hour of the day, or between that hour and ſun-ſet, the inſtruments being ſhaded from the ſun, and left a ſufficient time, (particularly the thermometers) to aſſume the temperature of the atmoſphere.

Barometrical

metrical Measurement of the Height of the Mole. *The Geometrical Measurement gives its Height*, 4211,3 *Feet, per Schuckburg.* Philos. Trans. 1777, p. 580 and 581.

MOLE.

w Barometer 28,131 Log. 4491852
p - - - 24,178 Log. 3834204

Difference = 657,648
Correction — 1,84

Logarithmic Result = 655,808 fathom.

Heat of the Mercury below 61°,8
- - - - - - - - above 57,2

Difference 4,6 × 4

Correction = 1,84

Calorific Result.

at the Air below 63°,9
- - - - above 56,

2)119,9(59,9

Mean = 59,9
— 32,

ental Difference = 27,9

First Product. 27,9 × ,0025 = 0,06975

Second Product. 0,06975 × 655,808 = 45,742

Error
4211,3
— 4209,3
= 2,0 feet.

Compleat Height.
655,808
+ 45,742
= 701,550 fathom × 6
= 4209,300 feet.

Summit of Schihallion, from General Roy. Phil. Trans. 1777, p. 775.

11th of July.

ower Barometer 29,595 Log. 4712183
pper - - - 26,194 Log. 4182018

Difference = 530,165
— 5,400

Logarithmic Result = 524,765

Heat of the Mercury below 59°,5
- - - - - - - above 46,

Difference = 13,5 × ,4

Correction = 5,40

Calorific Result.

t the air below 56,2
- - - above 45,

2)101,(50,5

Mean = 50,5
— 32,

ental Differ. = 18,5

First Product. 18,5 × ,0025 = 0,04625

Second Product. ,04625 × 524,76 = 24,270

Compleat Height.
524,765
+ 24,270
= 549,035 fath. × 6 = 3294,210 feet.

And by Trigonometrical Measurement = 3281,

Error of Barometer . . 13,210 feet.

HERE the error of my method is 13,21 feet. Schuckburg's error is 11,5 feet, and Roy's—2 feet. But in calculating the height from the obſervations taken the next day, July the 12th, Roy's error was . . 29,5 feet, Schuckburg's . . 19,7 feet, and mine . . 20,6 feet. Phil. Tranſ. 1778, p. 685.

HERE it is evident, that theſe errors aroſe from a faulty determination of the temperature of the atmoſphere, for the approximate height found by General Roy, July 11th, was lower than that found July 12th. On the 11th it was 3142 feet, and on the 12th it was 3145 feet, and yet the compleat height was more defective. By my calculation it was higher the ſecond day than on the firſt, and yet the error was *minus*, though *plus* on the firſt day.

I SHALL here add farther, that the error in calculating the height of Snowden, from the obſervations of Auguſt 7th, two hours P. M. is by Schuckburg's method 8,1 feet, by mine 9,7, by Roy's 3,7; but by the mean of nine obſervations on different days, Roy's error is 6,1; the calculation that gave the beſt reſult was made on obſervations at nine o'clock A. M. and the worſt were made on obſervations during a haze or fog, whether above or below; which ſhews that the errors of theſe calculations proceed chiefly from the diſorderly progreſſion of heat into the atmoſphere. Hence the winter ſeaſon is not well ſuited to them.

HAVING

HAVING given, p. 436, &c. the ſolution of ſeveral problems when the mean temperature of the atmoſphere is at 32°, I ſhall now ſhew how ſimilar reſults may be obtained when the mean temperature is above 32°.

PROBLEM 1ſt.

To find the height mercury would ſtand at in a barometer on a given elevation in a known latitude in ſummer and in clear weather, when the height at which it ſtands at the bottom of that elevation and the temperature of the atmoſphere below are alſo given.

SOLUTION 1°.

FROM the latitude deduce the height of the line of congelation, (ſee the table, p. 361,) and from the height of this line, and the temperature of the air below, deduce the temperature at the given height, as ſhewn p. 362. The temperature above muſt alſo be deemed that of the mercury in the upper barometer, as, if it were expoſed to the air a ſufficient time, it would certainly aſſume it.

2°. CONVERT the meaſure of the elevation into fathoms, and ſubſtract theſe fathoms from the logarithm of the height of

of the lower barometer; the remainder will give a logarithm which will denote the natural number, which expreſſes the height at which the mercury will ſtand in the upper barometer.

But, 3tio. The height of the mercury thus obtained is only that which it would have in temperature 32°. To obtain its height in the actual temperature above, we muſt add to the mercurial height thus found its $\frac{1}{960}$* part, multiplied into the fundamental difference, viz. the difference of the temperature above with 32°.

Note. The ſupplemental height will generally be obtained more eaſily by multiplying the fundamental difference into the logarithmic reſult, and dividing the product by 400. Or if Schuckburg's coefficient be preferred, by 411.

* This coefficient I take from that great treaſure of mathematical knowledge Dr. Hutton's Dictionary, article *Barometer*.

Examples.

Examples.

Saleve.

THIS mountain ſtands in the vicinity of Geneva and nearly in latitude 46°; its height over a certain point of the plain below was accurately meaſured geometrically by Sir George Schuckburg, and found to be 2831,3 feet. He alſo gave three ſeries of barometrical obſervations to aſcertain its height barometrically, from the ſecond of which ſeries the indications here given are taken. See Philoſ. Tranſ. 1777, p. 530.

HEIGHT of the lower barometer 28,390 inches.
Temperature of the atmoſphere below 73°.

THESE are the neceſſary *data*. The temperature above is part of the object of inveſtigation.

Inveſtigation of the Height of Mercury in the upper Barometer.

1°. THE temperature of the atmoſphere above, by the method given p. 362, was found to be 64,16 and by obſervation 64°, therefore I neglect the fraction as inconſiderable. This muſt alſo here be deemed to be the temperature of the mercury in the upper barometer, though by obſervation it was found higher,

for

for that was owing to accidental cauſes, viz. the heat of the tent in which it ſtood, and that of the man that carried it, and not having been allowed time to cool.

2do. 2831,3 feet converted into fathoms = 471,833 fathoms.

Log. of 28,390 = 4531,654

Subſtract the fathoms —471,833

Remainder = 4059,821

which is the logarithm of the height of the mercury in the upper barometer, and denotes the natural number and height of the mercury = 25,467 inches nearly.

But this height is that which the mercury would have in the temperature of 32°, and the temperature being 64° this height muſt be increaſed by $\frac{1}{9600}$ part multiplied into the fundamental difference, which in this caſe is 32°. Now $\frac{25\,467}{9600}$ = ,00265, which multiplied into 32° gives ,0901, and this added to the mercurial height 25,467 gives 25,557. its true height at temperature 64°.

In this manner the barometrical heights at the altitudes of the line of congelation may be found. Thus the altitude of the line of congelation, latitude o being 28000 = 4666,66 fathom, if

if the barometer below be *at* 30 *inches* we have log. 30 = 477121 —4666,66 = log. 010455, which denotes the natural number 10,244; and if the lower barometer be at 29,8 we have log. 474216—4666,66 = log. ,007550, which denotes the natural number 10,176.

And thus we find that the variations of the barometer at great heights are generally ſmaller than thoſe of the inferior barometer,* as De Luc and Sauſſure have alſo remarked.

Thus the variation of the inferior barometer in this caſe is 0,2 of an inch, and the correſpondent variation of the higher is only ,0068 of an inch.

Yet ſometimes from local cauſes which diſturb the natural progreſſion of heat, the variations of the barometrical height above are greater or ſmaller, than, proportionally to the barometrical height below, they ought to be; but the difference is inconſiderable, though important, when the meaſurement of heights is aimed at.

Problem II.

The height of mercury in a barometer at any elevation in the atmoſphere being given or found, and alſo the height of mercury in a barometer vertically below that height, to find the

* 7 Sauſſure, 8vo. § 2049, p. 388.

weight of the intermediate column of air, on a baſis of one ſquare inch, let the temperature be what it may.

SOLUTION.

THIS is found as in p. 439, and thus the weight of a column of air of any elevation and temperature, and under any given preſſion, may be known, firſt determining the height or number of inches at which mercury would ſtand on that elevation by the laſt problem.

THUS the weight of the intermediate column of air at Saleve, the barometer below being at 28,39 and above at 25,486, was 28,390—25,486 = 2,904 inches of mercury = 3443,2 grains × 2,904 = 9999,0528 grains or 20,810 troy ounces; whereas the weight at the mean temperature of 32° would have been 28,390—25,467 = 2,923 inches of mercury = 20,967 troy ounces.

Note.—The weight of a *vertical* column of air of a given length cannot be determined, unleſs the weight to which it is ſubject be known, and conſequently the height at which mercury would ſtand in a barometer placed over it be alſo known; now this weight is variable according to the ſtate of the barometer below, though the temperature ſhould be conſtant.

THUS

Thus ſuppoſing the length of the vertical column 1000 feet, the weight of theſe 1000 feet will be greater when the barometer below ſtands at 30 inches, than when it ſtood at 29 inches, and conſequently the height of the barometer to which they are ſubject will be proportionably diminiſhed —Thus in temperature 32°, barometer 30, a vertical column of air of 1000 feet in length, that is 83 3333 feet or 13,888 fathom, weighs 328,8 grains, for logarithm 30=4771,213 13,888 fathoms gives the log. 4757325, which correſponds with the natural number 29,9045 inches. And 30,0000 inches—29,9045 = ,0955 and ,0955 of an inch of mercury = 3443,2 grains × ,0955 = 328,8 grains.—But when the barometer below is at 29. a vertical column of 1000 inches will be found by a ſimilar proceſs to weigh 317,9 grains, the upper barometer being 28,90772.

Table of the Heights of Air in Feet, which correſpond with the Weight of one tenth of an Inch of Mercury or 344,32 *Grains. from Barometer* 31 *to* 15.* *Temperature* 32°.

Barometer.	Feet of Air.	Barometer.	Feet of Air.	Barometer.	Feet of Air.
31,—	——	29,4	88,5	27,8	93,6
30,9	84,2	,3	88,7	,7	93,9
,8	84,5	,2	89,1	,6	94,3
,7	84,7	,1	89,4	,5	94,5
,6	85,0	29,—	89,7	,4	95,0
,5	85,3	28,9	90,0	,3	95,2
,4	85,6	,8	90,3	,2	95,7
,3	85,8	,7	90,7	,1	95,9
,2	86,2	,6	90,9	27,—	96,4
,1	86,4	,5	91,3	26,9	96,7
30,—	86,7	,4	91,6	,8	97,0
29,9	87,0	,3	91,9	,7	97,4
,8	87,3	,2	92,2	,6	97,8
,7	87,6	,1	92,6	,5	98,1
,6	87,9	28,—	92,9	,4	98,6
,5	88,2	27,9	93,2	,3	98,8

Barometer.

*Taken from Sir George Shuckburgh's table. Phil. Tranſ. 1777, p. 586.

Barometer.	Feet of Air.	Barometer.	Feet of Air.	Barometer.	Feet of Air.
26,2	99,3	24,3	107,0	22,4	116,0
,1	99,7	,2	107,5	,3	116,6
26,—	99,8	,1	107,9	,2	117,1
25,9	100,6	24,—	108,3	,1	117,7
,8	100,8	23,9	108,8	22,—	118,1
,7	101,2	,8	109,3	21,9	118,8
,6	101,6	,7	109,7	,8	119,2
,5	102,0	,6	110,2	,7	119,8
,4	102,4	,5	110,6	,6	120,4
,3	102,8	,4	111,1	,5	120,9
,2	103,2	,3	111,6	,4	121,5
,1	103,6	,2	112,1	,3	122,0
25,—	104,0	,1	112,6	,2	122,6
24,9	104,4	23,—	113,0	,1	123,3
,8	104,9	22,9	113,6	21,—	123,7
,7	105,3	,8	114,0	20,9	124,4
,6	105,7	,7	114,5	,8	125,0
,5	106,1	,6	115,1	,7	125,6
,4	106,6	,5	115,6	,6	126,2

Barometer.	Feet of Air.	Barometer.	Feet of Air.	Barometer.	Feet of Air.
20,5	126,8	18,6	139,7	16,7	155,6
,4	127,4	,5	140,5	,6	156,5
,3	128,0	,4	141,2	,5	157,5
,2	128,7	,3	142,0	,4	158,4
,1	129,3	,2	142,8	,3	159,3
20,—	130,0	,1	143,6	,2	160,4
19,9	130,6	18,—	144,3	,1	161,3
,8	131,3	17,9	145,2	16,—	162,4
,7	131,9	,8	146,0	15,9	163,4
,6	132,6	,7	146,8	,8	164,4
,5	133,3	,6	147,6	,7	165,4
,4	134,2	,5	148,5	,6	166,5
,3	134,6	,4	149,3	,5	167,6
,2	135,3	,3	150,2	,4	168,6
,1	136,1	,2	151,1	,3	169,8
19,—	136,8	,1	151,9	,2	170,9
18,9	137,5	17,—	152,9	,1	172,0
,8	138,2	16,9	153,7	15,—	173,1
,7	139,0	,8	154,6		

Table

Table of the Heights of Air in Feet, reaching from Barometer 32 *Inches to Elevations where it would ſtand at one Inch, Temperature* 32°. *or the incorrect approximate Heights in other Temperatures.*

Barometer. Inches.	Feet.	Barometer. Inches.	Barometer. Inches.	Barometer. Inches.	Feet.
32,—	00,0	31,—4	1336,6	29,8	2745,4
31,9	81,6	,3	1422,4	,7	2836,1
,8	163,4	,2	1508,6	,6	2927,0
,7	245,1	,1	1595,0	,5	3018,3
,6	327,8	30,—	1681,7	,4	3109,9
,5	410,4	,9	1768,7	,3	3201,8
,4	493,2	,8	1856,0	,2	3294,0
,3	576,3	,7	1943,6	,1	3386,6
,2	659,7	,6	2031,5	28,—	3479,5
,1	743,4	,5	2119,7	,9	3572,7
31,—	827,3	,4	2208,2	,8	3666,3
,9	911,5	,3	2296,9	,7	3760,2
,8	996,0	,2	2386,0	,6	3854,5
,7	1080,7	,1	2475,4	,5	3949,0
,6	1165,7	29,—	2565,1	,4	4044,0
,5	1251,0	,9	2655,1	,3	4139,2

Barometer. Inches.	Feet.	Barometer. Inches.	Feet.	Barometer. Inches.	Feet.
28,2	4234,9	25,—	6432,6	23,8	8832,9
,1	4330,8	,9	6537,0	,7	8947,4
27,—	4427,2	,8	6641,9	,6	9062,5
,9	4523,9	,7	6747,2	,5	9178,1
,8	4620,9	,6	6852,9	,4	9294,1
,7	4718,3	,5	6959,0	,3	9410,7
,6	4816,1	,4	7065,6	,2	9527,8
,5	4914,2	,3	7172,6	,1	9645,5
,4	5012,8	,2	7280,1	22,—	9763,6
,3	5111,6	,1	7388,0	,9	9882,4
,2	5210,9	24,—	7496,3	,8	10001,6
,1	5310,6	,9	7605,1	,7	10121,4
26,—	5410,4	,8	7714,2	,6	10241,8
,9	5511,0	,7	7824,1	,5	10362,7
,8	5611,8	,6	7934,3	,4	10484,2
,7	5713,0	,5	8044,9	,3	10606,2
,6	5814,6	,4	8156,0	,2	10728,8
,5	5916,6	,3	8267,6	,1	10852,1
,4	6019,0	,2	8379,7	21,—	10975,8
,3	6121,8	,1	8492,3	,9	11100,2
,2	6225,0	23,—	8605,3	,8	11225,2
,1	6328,6	,9	8718,9	,7	11350,0

Barometer. Inches.	Feet.	Barometer. Inches.	Feet.	Barometer. Inches.	Feet.
20,6	11477,0	19,6	14138,2	17,6	17102,5
,5	11603,8	,5	14278,7	,5	17260,0
,4	11731,2	,4	14419,9	,4	17418,4
,3	11859,2	,3	14561,9	,3	17577,7
,2	11987,9	,2	14704,7	,2	17738,1
,1	12117,2	,1	14848,3	,1	17899,4
20,—	12247,2	18,—	14992,6	16,—	18061,8
,9	12337,0	,9	15137,8	,9	18225,2
,8	12509,1	,8	15283,8	,8	18339,6
,7	12641,0	,7	15430,6	,7	18558,0
,6	12773,6	,6	15578,2	,6	18721,5
,5	12906,9	,5	15726,7	,5	18889,1
,4	13041,1	,4	15876,0	,4	19057,7
,3	13175,6	,3	16026,2	,3	19227,5
,2	13310,9	,2	16177,3	,2	19398,4
,1	13447,0	,1	16329,2	,1	19570,4
19,—	13583,8	17,—	16482,1	Inches 15,—	19743,5
,9	13721,3	,9	16635,8	14	21541,3
,8	13859,5	,8	16790,4	13	23472,4
,7	13998,5	,7	16946,0	12	25558,1

Barometer. Inches.	Feet.	Barometer. Inches.	Table of different Feet.	Barometer. Inches.	Table of different Feet.
11	27825,4	30	000,0	19	11902,1
10	30309,0	29	883,4	18	13311,0
9	33054,4	28	1797,8	17	14800,4
8	36123,6	27	2745,5	16	16380,1
7	39603,1	26	3728,7	15	18061,8
6	43619,9	25	4751,0	14	19859,6
5	48370,8	24	5814,6	13	21790,7
4	54185,4	23	6923,6	12	23876,4
3	61681,8	22	8082,0	11	26143,7
2	72247,2	21	9294,2	10	28627,3
1	90309,0	20	10565,5		

Theſe two laſt columns give the length of a column of air at 32°, counting from barometer 30 to a height in which the barometer would ſtaud at (ſuppoſe) 29, = 883,4 feet.

Uſes

Uſes of this Table.

1°. To find the approximate height of any elevation in temperature 32°, the heights of the barometers above and below being given.

Substract the number of feet oppoſite to the lower barometrical height, from thoſe oppoſite to the upper, the remainder is the approximate height very nearly. For as only one decimal place is given, there is no room for a correction.

Example.

Thus at the bottom of Mount Saleve a barometer ſtood at 28,39 inches (ſay 28,4) and at the top at 25,7 inches. Now in the table we have oppoſite to 25,7 inches 5713,0 feet, and oppoſite to 28,4 we have 3109,9 feet. Now 5713,0 — 3109,9 = 2603,1 feet, and Schuckburg makes it =2602,878 feet.

Supplemental Height.

Multiply the fundamental difference of heat, into the approximate height, and divide the product by 450, if the height does not exceed 8000 feet; if it does, 1140 ſhould be the diviſor, the quotient is the ſupplemental height.

Thus in the above caſe the fundamental difference was 37,4, and the approximate height is 2603,1 feet. Now $\frac{37,4 \times 2603,1}{450} =$ 219,8, which added to 2603,1 = 2819 feet.

The error is 12 feet, and ſcarce ever exceeds by 40 feet the reſults of other methods.

2d°. To find the height or number of inches at which mercury would ſtand on a given elevation true to five-tenths of an inch, when that altitude does not exceed 19700 feet, the height of the mercury below being given.—Add to the given altitude, that oppoſite to the given barometrical height. The altitude neareſt to the ſum of both will point out the barometrical height ſought.

Thus the height of Mount Blanc being 14624 feet above Mr. Senebier's cabinet at Geneva, and the height of the lower barometer being 29,1 (Engliſh), a barometer at the top of that mountain was found to ſtand at 17 inches—And by this calculation it ſtood at 16,6, for oppoſite to 29,1 we have 2475. And 14624+2475=17099. The neareſt tabular number to this is 17102, which denotes 16,6 inches of mercury. The error then is 0,3 of an inch.—But in general it is much ſmaller; thus, in meaſuring the Mole, whoſe height is 4211 feet, Schuckburg found the lower barometer 28,1, &c.

&c. and the upper 24,1. Now in the table the oppoſite number to 28,1 is 3386, which added to 4211 gives 7597, the neareſt tabular number is 7605, which denotes the mercurial height 23,9, the error then is only 0,2 of an inch.

3tio. The barometrical height below being given, to find on what elevation the barometer above ſhould ſtand at a given number of inches, temperature 32°. This is in effect to ſeek the approximate height of that elevation, and therefore the elevation is found in the ſame manner. Thus the barometer below being at 28,4, the elevation at which it would ſtand at 25,7 is 2603 feet.

Note.—The table commencing at 30, I have inſerted becauſe we generally reckon upwards from the level of the ſea, at which the mean mercurial height, temperature 32°, is 30 inches. The intermediate tenths muſt be found by ſubſtraction from logarithm 30. Thus the height of mercury 29,1 denotes an elevation of 793 feet.

CHAP.

CHAP. V.

Of Precipitations from the Atmoſphere.

THERE are five ſubſtances conſtantly contained in the atmoſphere, or at leaſt in its lower ſtrata, with which we are principally concerned; namely, oxygen, mephite, moiſture, caloric, and electron, otherwiſe called the electric fluid, if this be not (which nevertheleſs I ſuſpect it to be) a modification of caloric.* As, however, its effects are very different from thoſe uſually aſcribed to caloric, it may and ought, like ice and water, to be diſtinguiſhby a different appellation. Beſides theſe, heavy inflammable air and other miaſmata are frequently found in it, and the lighter inflammabie air in its ſuperior ſtrata; but theſe mixtures are contingent, and therefore form no part of the preſent general inquiry.

Oxygen is frequently precipitated through its affinity to terreſtrial ſubſtances in certain ſtates, as combuſtion, putrefaction, &c. Effects

* By *modification* I here mean a different ſituation of caloric, namely, on the external ſurfaces of bodies, and not in their minuteſt internal cavities. *Light* I take to be another modification of caloric, namely, an homogeneous arrangement of ſome or all its conſtituent particles, which, when thus arranged, and then only, become *viſible*.

fects foreign to meteorology. In the atmoſphere, meeting with inflammable air, as in thunder ſtorms, it is often converted into water by the electric explofion, and thus precipitated; and thus moſt *fiery* meteors are formed.

THE *electric fluid* is generally precipitated either ſuddenly during electric explofions, or gradually and ſilently by the diminution of caloric and the fall of *dew* or *rain*. It is theſe laſt precipitations which I now mean to examine; and as I aſcribe them to electric agency, it will be neceſſary to lay down thoſe principles of atmoſpheric electricity which are moſt generally adopted by electricians, and the conſequences that appear to me clearly deducible from them.

SECTION I.

Of Atmoſpheric Electricity.

1°. MERE atmoſpheric air is an *idio-electric*, or non-conducting ſubſtance.

2°. AIR holding water in ſolution is alſo a non-conductor, and ſo much the *more perfectly ſo*, as, relatively to the degree of heat it poſſeſſes, it is farther removed from the point of ſaturation, and

and ſo much the leſs perfect as it is nearer to ſaturation with moiſture.

3°. As heat increaſes, and cold diminiſhes the ſolvent power of air, it follows that the quantity of moiſture in both caſes remaining the ſame, its non-conducting power is in direct proportion to its heat.

4°. MERE vapour is a *conductor* electricity, and ſo much the more perfect as it is *hotter*; and moreover, it contains and appropriates a conſiderable proportion of the electric fluid.

5°. VAPOUR diſſolved in air forms, as already ſaid, a compound which poſſeſſes the non-conducting property in the direct ratio of its heat and the inverſe of its proximity to ſaturation; this compound involves alſo a conſiderable portion of the electric fluid proportioned to the heat of the vapour in its naſcent ſtate.

6°. VAPOURS ſeparated from air, through whatever cauſe, form *clouds* and *foggs*, as already mentioned. In the firſt ſtage of their coaleſcence, the minute particles of which theſe conſiſt, retain, in the form of electric atmoſpheres ſurrounding each particle, a conſiderable proportion of the electric mattar which they previouſly, involved in their combined ſtate, and ſo much the greater, as they contained

contained more of it in that ſtate; but in this ſtate of coaleſcence they *retain* the electric fluid more ſlightly and become *conductors*, inſulated by the drier air that ſurrounds them.

7°. As the various ſubſtances which form the ſurface of the globe are variouſly heated, ſo alſo are the vapours they emit, and which are united to the air incumbent over them, conſequently they involve, when united to air, different portions of the electric fluid.

Conſequently 1°. The vapours emitted in different latitudes contain different portions of electric matter, bearing a porportion to the temperatures of thoſe latitudes.

2°. So alſo do the vapours emitted at different ſeaſons of the year.

3°. So alſo thoſe emitted from the ſea, and thoſe emitted from land between the tropics and the warmer tracts, if not mountainous; the former contain leaſt, and the latter moſt of the electric fluid.

4°. In the colder regions, in ſummer, the marine vapours alſo contain leaſt, and thoſe from land contain moſt of this fluid. But in winter the marine contain moſt, and thoſe from land leaſt.

5°. For the ſame reaſon the vapours that originate from high mountains contain leaſt, and thoſe from plains moſt, and thoſe alſo may vary in different ſituations.

8°. Hence we ſee that *clouds* originating from the condenſation of thoſe vapours, and each retaining, though more looſely, at leaſt a portion of the electric fluid they originally involved, muſt contain different portions of it, or be, what is called, differently electrified, that is, *plus* or *minus*, at leaſt relatively to each other.

9°. Clouds of equal ſurfaces and ſimilarly and equally electrified repel each other. But if the ſurfaces be very unequal, the greater will gradually ſteal the electron from the ſmaller if brought into contact, as *Coulomb* has ſhewn with reſpect to metallic balls ſimilarly electrified. Mem. Paris, 1787. If near, but not in contact, the greater will throw the leſſer into a contrary ſtate and then attract it.

10°. Clouds diſſimilarly electrified, and beyond the ſtriking diſtance, attract each other.

11°. So alſo portions of air impregnated, but often far from being ſaturated with vapours unequally electrified, act upon each other, thoſe more ſtrongly electrified robbing the weaker of a portion

portion of their electron, and thus these are, at least in part, converted into a *cloud*; but clouds being still better conductors of electricity, the cloud thus formed robs, in its turn, the portions of air more strongly electrified of their electron, and converts, at least partially, their vapours into clouds; thus the dimensions of the original cloud is increased until it covers a considerable part of the horizon. By this sudden nubification a considerable quantity of caloric is set loose, which rarefies the air, and thus occasions a storm or erruption of air from all quarters, in a word, a *hurricane*.

Section II.

Of Dew.

In hot weather, particularly if the heat is long continued, as in summer and autumn, vapours are dissolved and elevated to great heights, but in the evening, particularly after sun-set, when the air is cooled, a portion of this vapour, proportioned to the refrigeration, gradually descends, until at last the lower strata of air become saturate and gradually deposit it on the contiguous denser substances. The moisture thus deposited forms what is called *dew*. That there is likewise an ascending dew arising from the effluvia of vegetables which emit air accompanied with moisture, but deposit it on the inferior surfaces of more elevated

ſubſtances, has been ſufficiently proved and explained by Muſchenbroeck and Du Fay. Mem. Paris 1756.

THERE is however a puzzling circumſtance mentioned by the above-named naturaliſts, and for which they have not accounted. It has been obſerved in ſeveral places to be depoſited very copiously on idioelectrics, as glaſs and all vitrifactions, but ſcarce at all on the beſt conductors, as metallic ſubſtances, which evidently ſhews that dew conveys electricity; but why is not this fluid carried off by conducting ſubſtances more eaſily than by non-conductors? Mr. Achard tells us, Mem. Berlin, 1780, p. 17, that conductors aſſume the electrical ſtate of the ambient air very eaſily, but glaſs very ſlowly; therefore the conductors and vapours being both in the ſame ſtate, whether poſitive or negative, repel each other, but glaſs being in a contrary ſtate repels them. This, however, does not appear to me perfectly ſatisfactory, as far as reſpects glaſs, for it ſhould ſeem that, after ſome hours, this alſo ſhould aſſume the ſame electrical ſtate as the air, and then repel the vapours. I ſhould rather ſuppoſe that glaſs collects moiſture by reaſon of its ſtrong attraction for moiſture, for it has been obſerved that, in diſtilling water in glaſs and metallic veſſels of the ſame dimenſions and in the ſame heat, much more water paſſes in a given time through the glaſs than through the metallic veſſels: nay when the helm of a glaſs ſtill was connected with two receivers,

ceivers, with one by a metallic and with the other by a glaſs adopter, more paſſed through the latter than through the former. It is probable that Du Fay, who made theſe experiments, would give this explanation if he had purſued this inquiry. Thus the breath is immediately evaporated from poliſhed ſteel, but is retained by poliſhed glaſs.

Muschenbroeck alſo obſerves that this different power of collecting dew is in ſome meaſure local, for that it does not take place at Leyden though it does at Utrecht, Heſs and Paris, which appears to me to be occaſioned by the unequal electrization of the vapours; thoſe from the vicinity of Paris and Utrecht being collected from a drier ſoil are more highly electrified than thoſe of Leyden, the ſoil of which is ſo moiſt that they can have no cellars, whereas they can and have at Utrecht. He alſo tells us, fogs, univerſally, equally moiſten all bodies, which ſhews they contain much leſs electron than deſcending vapors.

Hence dew is nothing more than the condenſed vapours elevated during the day from the ſoil over which the air that depoſits it is incumbent, and hence its noxious qualities when elevated from ſtagnating marſhes, and the various impregnations it has been found to contain.

Section

Section III.

Of the Haze of the Year 1783.

This year is remarkable for ſeveral extraordinary phœnomena of which this haze was the moſt univerſal. It is but ſlightly mentioned by Engliſh meteorologiſts*, but frequently by thoſe on the continent, by whom it has been variouſly accounted for. The facts that relate to it, or appear to me to do ſo, are the following:

1°. In the months of February and March of that year happened the great earthquakes of Calabria, ſo minutely and exactly traced and deſcribed by Sir William Hamilton. Phil. Tranſ. 1783, p. 170, &c.

2°. During this earthquake the incumbent atmoſphere was incumbered and obſcured by clouds to an uncommon degree. 24 Roz. Journ. p. 5.

3°. On the 17th and 18th of the ſubſequent month of June a dark reddiſh haze that obſcured the light of the ſun was obſerved in many parts of Europe, even in high northern latitudes as England and Sweden, Phil. Tranſ. 1784. p. 285, 418, and 24 Roz. p. 8 and 405.

4°.

* Phil. Tranſ. 1784.

4°. This haze was in moſt places introduced by a ſouth or ſouth eaſt wind. 24 Roz. p. 5 and 409. Mem. Dijon, 1783, p. 228.

5°. This ſoutherly wind did not produce rain, nor lower the barometer, but on the contrary raiſed it to an unuſual height, and it fell when the wind changed to the north. 24 Roz. p. 4. 5 Neve Schwd. Abhandl. p. 9 and 13. 2 Mem. Dijon, 1783 p. 214. 1 Mem. Lauſanne, p. 116 and 280.

6o. This ſoutherly wind when the haze firſt appeared occaſioned an extraordinary degree of cold, for the month of June. 23 Roz. p. 203; 24. Roz. p. 4. Phil. Tranſ. 1784, 417 and 418.

7°. This haze at firſt ſtood at an extraordinary height, in ſo much that it was not diſſipated or leſſened by rains or winds, but ſeemed of a dry nature; but after ſome time it gradually ſunk lower, and was at leaſt partially diſſipated by rains and winds. Phil. Tranſ. 1784, p. 285. 24 Roz. p. 4, 5, 9, 10, 407 and 408. 1 Mem. Dijon, 229. 1 Mem. Lauſan. 119.

8°. It was followed at certain intervals by thunder and ſhort but copious falls of rain, and fiery meteors, one of which is well traced and deſcribed by Sir Charles Blagden. See Phil. Tranſ. 1784, p. 201, 285 and 286. 1 Mem. Lauſanne, 114. 23 Roz. 203. 24 Roz. 5, 10, and 409.

9°. It

9°. It is even ſaid to have depoſited in ſome places a viſcid liquid of a diſagreeable taſte, 24 Roz. 20. 1 Mem. Dijon, 1783, p. 229. Nay ſome ſay it was accompanied by an acrid or ſulphureous ſmell, 23 Roz. Journ. p. 203, and 24 Roz. p. 410.

Other circumſtances are mentioned by the authors quoted, but with ſome variation, ſuch as muſt be expected to ariſe from different ſituations as well as from the different ſtates of this miſt.

From an attentive conſideration of theſe obſervations I am diſpoſed to concur with the opinion of Sir William Hamilton, Phil. Tranſ. 1783, p. 194 and 199. That this haze was cauſed by the immenſe quantity of inflammable air extricated from the bowels of the earth during the earthquakes of Calabria, ſtrongly electrified, and impregnated with ſulphureous bituminous, earthy and metallic particles. The quantity was ſuch as to diffuſe itſelf after a few months over moſt parts of Europe. While theſe heterogenous particles were held in ſolution the tranſparency of the atmoſphere was not altered; it was otherwiſe when they began to precipitate.

Then 1°. The obſcurity and dark red colour of the haze may be attributed to the ſulphureous metallic, &c. particles, which abſorbed all but the leaſt refrangible ſolar rays, particularly at ſunriſe and ſun-ſet.

2do. The

2do. The direction of the wind ſeems merely a contingent circumſtance, yet as this phœnomenon, or at leaſt its cauſe, originated in the ſouthern part of Europe, it poſſibly has been extended northwards by a ſouthern wind.

3°. While the *impetus* of the eruption of this air laſted, it may well be ſuppoſed to reach the greateſt heights like the ſtill denſer ſmoke of volcanos; but, as it conſiſted chiefly of heavy inflammable air, it may be inferred that it gradually deſcended and its heterogeneous contents became viſible every where nearly at the ſame time, but its height for a long time ſecured it from the winds that prevail in the lower ſtrata of the atmoſphere; but when it had deſcended ſome way theſe winds and rain gradually diſperſed and waſhed it away.

4°. The atmoſphere, thus loaded with metallic and other particles thrown into it far beyond the limits ſuited to their ſpecific gravity, muſt have its weight increaſed, and hence the extraordinary heights of mercury in barometers. While diffuſed over vaſt ſpaces, the additional weight accruing to the atmoſphere from theſe particles may have been inſenſible or not eaſily diſtinguiſhed, but became evident when they had ſomewhat deſcended and were concentrated, and hence the varying accounts we have had of the manner in which barometers were affected during the miſt;

neither can it be ſuppoſed that the denſity of theſe foreign particles was ever the ſame.

5°. The uncommon cold produced by this haze originated, partly from the obſtruction it preſented to the paſſage of the ſolar rays, and partly from the deſcent of its particles exceedingly cooled in the lofty regions of the atmoſphere into which they at firſt aſcended; this effect, however, muſt have varied with their number, denſity, and the duration of their precipitation.

6°. The thunder, fiery meteors and the ſudden and violent rains, which accompanied the deſcent of this haze, may well be attributed to the high electrical ſtate of its particles which were inſulated in the more elevated regions, but was often communicated in the lower and moiſter ſtrata; on this occaſion the bituminous and ſulphureous particles muſt have acted a principal part.

A similar haze was obſerved in Perſia in the year 1721, after the great earthquake in Georgia which deſtroyed the city of Tauris. 5 Richard's Hiſt: des meteors, p. 164. The darkneſs which obſcured the ſun after the death of Cæſar may probably be aſſigned to a ſimilar cauſe, for Julius Obſequens * de Prodigiis, tell us there were earthquakes about that time.

The

* He lived in the reign of the emperor Honorius, and collected the prodigies mentioned by Livy, &c.

The only objection to this theory, which may at firſt view appear plauſible, is, that the quantity of ſmoke elevated to the greateſt height during volcanic eruptions does not produce a haze of a ſimilar nature and extent; yet on conſideration it appears to me of no weight; for, 1° The electricity of this ſmoke is deſtroyed during the eruption by the frequent flaſhes of lightning that take place in the body of the ſmoke during its aſcent, and conſume the hydrogen and bituminous particles; and, 2do. This ſmoke then conſiſts chiefly of heated air, impregnated with incinerated carbonic particles, and not of metallic particles of which it is not a ſolvent; and as the colder ambient air preſſes towards the volcano and the heated ſmoke, its diffuſion is prevented, and never, unleſs by a ſtorm, are its particles carried farther than 20 or 30 miles. See Hiſtoire de Veſuve par Padre della Torre, p. 127, &c.

Section, IV.

Of Rain.

Diminution of the temperature of air, replete with moiſture, below the degree at which its ſaturation takes place, whether this refrigeration were cauſed by rarefaction, or by the intermixture with colder air, has been generally ſuppoſed the cauſe of rain; but this hypotheſis in both its branches

has been ſatisfactorily refuted: the firſt by Sauſſure* and the ſecond by De Luc†. And in fact refrigeration will indeed ſeparate moiſture from air cooled below the temperature neceſſary to hold it in ſolution. But this ſeparation will terminate, if gradual, in the production of *dew*, as already ſeen; or, if ſudden, in the production of a *cloud*, as in the experiment of Torneä mentioned by Maupertuis, but cannot nor has in any inſtance produced *rain*.

Rain is the immediate reſult of the union of the particles which form clouds; and this union is the conſequence of the ſubtraction of the electric atmoſpheres which keep them at a diſtance from each other; and this ſubſtraction is itſelf the conſequence either of the ſuperior attraction of better conductors or of the attraction or repulſion of other clouds through the cauſes mentioned in the firſt ſection of this chapter. The conſequence of the *attraction* of clouds is their *incorporation*, and the reſult of their incorporation is the increaſed volume of their conſtituent particles, an increaſe proportioned to the attraction that produced it; the increaſed volumes, thus produced, form thoſe drops whoſe collection we call *rain*. The weight of theſe being ſuperior to the reſiſtance of air, they neceſſarily deſcend, and the cauſe of their different ſize is thus clearly diſcerned.

The

* Hygrom. § 224. † De Luc Idées de Meteorologie, p. 43, &c.

The repulſion of clouds ſimilarly electrified, and not greatly differing in magnitude, terminates in a bare increaſe of diſtance; but, if their magnitudes be much diſproportioned, it may terminate in *attraction*, or at leaſt in forcing the conſtituent particles into cloſer contact, and thus by increaſing their magnitude effect the ſame reſult.

When the attraction takes place between clouds differently and highly electrified, and within what electricians call the *ſtriking diſtance*, the electric fluid is ſet free, the coaleſcence of the nubilous particles is more rapid and complete, and hence the large drops that follow flaſhes of lightning, or even floods, where the quantities both of vapour and electron are conſiderable, as between the tropics.

Upon theſe principles moſt of the phonomena relative to rain appear to me eaſily explicable; of theſe the moſt remarkable are:

1°. That rains are more copious but leſs frequent in the ſouthern parts of our hemiſphere not much elevated over the ſea, than in the more northern latitudes. They are more copious when their productive cauſes occur, evidently becauſe the quantity of ſuſpended vapour is much greater in the hotter than in the

the colder regions; but they are leſs frequent, becauſe the variations of wind in different directions which introduce and intermix clouds differently electrified are leſs frequent; this might be proved by inſtancing the rainy ſeaſons between the tropics, were it not that this illuſtration would extend this paper to too great a length. Even in moderately elevated ſituations between the tropics, if inſulated and of ſmall extent, as the iſland of St. Helena, it ſeldom rains.

2do. That, in the temperate latitudes, rains are alſo more copious, though commonly leſs frequent in *ſummer* than in *winter*, for the reaſons already aſſigned. *Dry* ſummers are then the conſequence of uniform winds, from whatever quarter they may blow, as *wet* ſummers are of their variation, particularly if in oppoſite directions, and if they reach heights ſufficient to intermix the clouds that ſubſiſted during the reign of their antagoniſts.

3do. Southerly winds are moſt frequently accompanied with rain in moſt parts of Europe at leaſt and probably in moſt parts of our hemiſphere; but *northerly* and *eaſterly*, with clear, dry, and ſerene weather. Becauſe ſoutherly winds are not only warmer, proceeding from warmer climates, but alſo more highly electrified than the ſoil of the colder countries into which they flow. Hence the copious vapours they contain are quickly deprived of part of their electron, and thus converted into clouds; but the

the ſuperior ſtrata of the atmoſphere under which the ſouthern air is introduced, not being ſupported by air as denſe as that which ſubſiſted under them before their introduction, neceſſarily deſcend and mix with the inferior ſouthern air; by this intermixture they are warmed, and deprive the clouds already formed and in its vicinity of part of their electron, or perhaps in conformity to the eleventh principle, they are themſelves deprived of part of their electron by thoſe clouds, and the vapours they contain are thus converted into clouds; in either way clouds differently electrified muſt be formed. Hence proceeds their gradual attraction to each other which terminates in thoſe gentle ſhowers that uſually accompany this wind. Northerly and eaſterly winds on the contrary, proceeding from colder countries are leſs highly electrified than the ſoil of the countries they invade; and hence from the oppoſite reaſons to thoſe juſt mentioned they introduce ſerene weather and a diſpoſition adverſe to nubification.

The reaſons hitherto adduced to explain the different effects of theſe different winds, evidently aroſe from an ignorance of the origin and progreſs of theſe winds. It was imagined that ſoutherly winds, flowing into colder countries were ſuddenly cooled by an intermixture with the colder air of thoſe countries, and that thus their vapours were condenſed into rain, yet, even ſo, this intermixture could only produce clouds and not rain, but

but in fact this intermixture cannot take place, except with the ſuperior and unmoved ſtrata of the atmoſphere, and theſe alone could not produce numerous clouds, much leſs copious rains; for the air of the countries into which theſe ſoutherly winds flow, muſt itſelf have flown northwards, before the more ſouthern air could enter upon them, as ſhewn p. 397.

Moreover, ſoutherly winds retain much warmth, and northerly winds are ſo much colder in the countries into which they are introduced, that their temperature cannot be ſuppoſed ſufficiently altered to depoſit much vapour in the one caſe, or diſſolve much of that already condenſed in the other; on the contrary the warm ſoutherly wind ſhould diſſolve the clouds already formed, and the northerly, by their encreaſed cold, ſhould produce many more.

Hence electrical agency muſt of neceſſity be recurred to, though I do not doubt but it may be more correctly applied by perſons better verſed in electrical knowledge than I can pretend to be. Currents of air flowing in different directions at different heights in the atmoſphere, muſt undoubtedly be intimately connected with theſe effects, but with theſe we are at preſent too little acquainted.

4°. That a diſpoſition to rain is generally connected with a diminution of the weight of the atmoſphere, as is a diſpoſition to

to ſerenity with the increaſe of its weight. Becauſe under the diminiſhed weight of the atmoſphere, the eruption of vapours both from land and water is much more copious, a diſpoſition highly favourable to nubification, and the clouds already formed deſcend lower, are more concentrated, and hence more diſpoſed to react upon and attract each other and thus produce rain. The increaſed weight of the atmoſphere muſt produce oppoſite effects and induce a diſpoſition adverſe to the production of rain.

5°. That more rain falls on the ſurface of the earth than on ſmall elevations above it, as fron 30 to ſome 100 feet: ſee Phil. Tranſ. 1769, p. 361; and of 1771, 297; and of 1777, p. 256. This effect ſeems to me to proceed from the greater ſtillneſs and tranquillity of the air near the ſurface of the earth than at greater elevations. To prove this, it is only neceſſaty to collect the rain that falls in moderate weather on both ſituations, with that which falls on both, in more ſtormy weather. If this explanation be juſt the difference between the quantities collected in both ſituations will be found greater in the latter than in the former caſe. This experiment I ſhall make, and communicate the reſult to the Academy.

6°. That the quantities of rain collected at the top of high mountains, and on plains about half a mile diſtant from thoſe mountains are nearly equal, but in ſummer there falls ſomewhat

more on the plains, and in winter ſomewhat leſs. Phil. Tranſ. 1771, p. 295. The greater quantity of rain collected in ſummer on plains appears to me to proceed from the cauſe juſt mentioned, the leſs diſturbed ſtate of the atmoſphere; but the quantity of rain gained through the influence of this cauſe is often, in great meaſure, compenſated by that ariſing from the condenſation of fogs formed on the ſummits of mountains, particularly at night, when neither fog nor rain exiſt on the plains. But in winter, theſe miſts being much more frequent and denſer on the ſummits of mountains, the quantity of moiſture which they depoſit is far more conſiderable.

7°. That it rains much more on the weſtern coaſts of moſt parts of Europe, particularly if mountainous, than in the interior parts of thoſe countries, or on the eaſtern coaſts of the Britannic iſlands.

The cauſe of theſe phenomena is very obvious. Weſterly winds are by far the moſt frequent in moſt parts of Europe; theſe flow from the Atlantic which bounds it, and generally convey marine clouds electrified differently from the ſoil or land over which they flow, as alſo from that of the higher clouds under which they reign. Hence proceeds their mutual attraction, and thence rain. This effect muſt take place principally on the weſtern coaſts; when they

they proceed further, this different electrical ſtate muſt either ceaſe or be diminiſhed. When the coaſts are mountainous, theſe mountains quickly abſorb the electric matter contained in the weſtern blaſts, and by colliſion, condenſe the vapours they contain, firſt into clouds, and finally into rain. Hence it often happens that weſterly winds, particularly in ſummer, produce no rain, either becauſe they introduce no clouds, or meet with none differently electrified.

8°. That in ſome countries it ſcarce ever rains.—This ariſes from local circumſtances, as is apparent in the following inſtances: 1°. It never rains on the plains of Peru from the gulf of Guyaquil, nearly under the equator, up to latitude 23° ſouth, nor is thunder ever heard there, though theſe plains border on the Pacific Ocean, but they receive a ſlight dew every night. Bouguer, fig. de la Terre XXIII. 2 Ulloa's Mem. p. 157. 2 Phil. Tranſ. Abr. 132. Plainly from the following reaſons: Theſe plains are entirely ſandy, and, conſequently, emit very little vapour, being ſoon parched by the heat that there prevails; conſequently, the intermixture of marine vapours can produce no effect. Again, the clouds in theſe tracts are elevated to a great height, and are attracted by the electrical agency of the Cordellierres that border on theſe plains, to their lofty ſummits, and there produce copious rains; hence alſo the ſandy and extenſive deſarts of Arabia and Africa are ſeldom re-

freſhed by rain. 2do. It ſcarce ever rains in Egypt, particularly in Upper Egypt. Now it is to be obſerved, that Egypt is ſo ſituated betwixt lofty mountains that no wind can enter it without paſſing over them, but the northerly winds, which iſſue from the Mediterranean; for a ſoutherly wind muſt paſs over the mountains of Abyſſinia; an eaſterly, over thoſe that intercede between the Red Sea and the Nile, and proceeding from the Deſarts of Arabia, can convey little or no vapour; and weſterly winds muſt paſs over the Deſarts of Africa and Mount Atlas. Now the northerly wind does not begin to blow until the month of June, when Egypt is ſo ſcorched as to emit ſcarce any vapour, and the few clouds it may convey are attracted by the mountains of Abyſſinia. Towards the middle of June the inundation of the Nile, it is true, commences, and then, as the northerly wind ſtill continues, perhaps rain might be expected, but little attention being paid to it then, we are not informed whether any falls or not, perhaps the clouds then alſo paſs to the mountains of Abyſſinia, whither this wind conveys them, and which they deluge with rain; all other winds depoſit their moiſture on the reverſe of the mountains they paſs over.

CHAP.

CHAP. VI.

Prognoſtics.

When the barometer falls, and the hygrometer riſes, *rain* is announced.

When the barometer riſes, aud the hygrometer falls, we may expect fair weather, if farther changes do not appear in theſe inſtruments, as ſometimes there ſuddenly do.

If the barometer falls and the hygrometer alſo, *windy weather* will probably follow—particularly if the barometer falls much below its natural height, which in Dublin is from 29,9 to 29,98.

Again, in the morning the hygrometer is generally higher than at noon, by reaſon of the difference of temperature; but if it ſtands *lower at noon* in a greater proportion than the difference of temperature demands, it prognoſticates *fair weather*.—On the contrary, if at noon it be higher than it ſtood in the morning, *rain* may be expected. Sauſſ. Hygr. p. 356.

To foreſee the Riſe or Fall of the Barometer in Day-time.

OBSERVE it at ſeven o'clock in the morning, and afterwards at nine and at ten. If it remains ſteady, its next motion will probably be *downwards*. So alſo if it falls within that interval of time, the probability is, that it will *ſink* ſtill lower. But if it riſes within that interval, the chances of a greater riſe or of a greater fall are equal.

AGAIN, obſerve the barometer at one in the afternoon, and again at three; if it remains unmoved, it is probable that it will *riſe*, but if it has fallen, the chances of a farther riſe or fall are equal.

APPENDIX.

APPENDIX.

Of the Manner of taking Obſervations on the Hygrometer and of correcting its Indications.

THE hygrometer ſhould habitually be kept looſe under a glaſs jar, to prevent it from being fouled by flies or duſt. Obſervations ſhould be made in the open air and in the ſhade at a diſtance from houſes or reflected heat. It requires but about two minutes to arrive at its proper height. But the obſervations thus taken muſt be corrected by the table that follows:

TABLE

TABLE III.

Of the Degrees of Cold *neceſſary to bring the Hydrometer from its actual State to the Degree which marks Saturation.*

Hygrometer.	Cold.	Hygrometer.	Cold.	Hygrometer.	Cold.
XL	34,695	LV	21,825	LXX	12,846
XLI	33,642	LVI	21,135	LXXI	12,333
XLII	32,625	LVII	20,461	LXXII	11,829
XLIII	31,641	LVIII	19,801	LXXIII	11,333
XLIV	30,689	LIX	19,156	LXXIV	10,846
XLV	29,766	LX	18,524	LXXV	10,370
XLVI	28,870	LXI	17,905	LXXVI	9,904
XLVII	28,001	LXII	17,299	LXXVII	9,449
XLVIII	27,156	LXIII	16,705	LXXVIII	9,005
XLIX	26,334	LXIV	16,122	LXXIX	8,572
L	25,534	LXV	15,551	LXXX	8,151
LI	24,755	LXVI	14,990	LXXXI	7,741
LII	23,995	LXVII	14,439	LXXXII	7,340
LIII	23,254	LXVIII	13,899	LXXXIII	6,947
LIV	22,531	LXIX	13,368	LXXXIV	6,561

Hygrometer.	Cold.	Hygrometer.	Cold.	Hygrometer.	Cold.
LXXXV	6,182	XC	4,379	XCV	2,698
LXXXVI	5,810	XCI	4,034	XCVI	2,373
LXXXVII	5,444	XCII	3,694	XCVII	1,961
LXXXVIII	5,084	XCIII	3,358	XCVIII	1,399
LXXXIX	4,729	XCIV	3,026	XCIX	0,755

Notes—1°. THE degrees of heat here mentioned are thoſe of Reaumur.

2do. THAT the point of ſaturation is here ſuppoſed to be C.

3tio. THAT the ſecond column expreſſes the diſtance from ſaturation, that is, the number of degrees by which the heat muſt be diminiſhed to raiſe the hygrometer to C. As the degrees here mentioned are thoſe of Reaumur, theſe and thoſe of Fahrenheit muſt be converted into each other as occaſion may require*.

* To convert Fahrenheit's degrees into thoſe of Reaumur, when higher than 32°, ſubſtract 32, and divide the remainder by 2,23, the quotient is the degree of Reaumur on the *Geneva Thermometers*.—If the degrees of Fahr. be below 32, ſubſtract them from 32 and divide the remainder by 2,23.

To convert the degrees of Reaumur into thoſe of Fahr. if above o, multiply them into 2,23 and add 32 to the product.—If below o, multiply them into 2,23 and ſubſtract the product from 32, the remainder expreſſes the degree of Fahrenheit.

Of the Uſes of the foregoing Table.

The uſe of this table is to diſcover, when different obſervations are made at different times or places and at different degrees of heat, what the real quantity of moiſture is, as pointed out by the hygrometer; for as this inſtrument equally *deſcends* to *dryneſs* when the heat increaſes, or the quantity of vapours in the air decreaſes, and equally *aſcends to moiſture* when the heat *decreaſes*, or the quantity of vapours in the air *increaſes*; it is neceſſary to find on which of theſe cauſes, and to what degree, this variation depends. Now theſe problems may eaſily be reſolved by this table. I ſhall begin by the circumſtances of an *increaſe of heat*.

If the heat increaſes after the firſt obſervation, and the hygrometer deſcends, three caſes may have happened, 1°. There may have been no *variation of vapour*; or, 2do. There may have been an *increaſe* of vapour but diſproportionate to the heat; or, 3tio. There may have been alſo a *decreaſe* of vapour as well as an increaſe of heat.—We may alſo imagine a fourth caſe, in which there may be an increaſe of vapour proportionate to the increaſe of heat, but in that caſe the hygrometer would not deſcend.

The firſt ſtep to be taken in this inveſtigation is to obſerve what effect the increaſed degrees of heat ſhould have on the hygrometer, abſtracting

abſtracting from the influence of vapour. If the deſcent of the hygrometer be ſuch as the table ſhews to reſult from the mere increaſe of heat, then there was no increaſe or decreaſe of vapour; but if the deſcent be *ſmaller* than mere heat would occaſion, then there muſt have been an *increaſe* of vapour; and if the deſcent be *greater*, then there muſt have been alſo a *decreaſe* of vapour as well as an *increaſe* of heat.

Thus, 1°. Suppoſe an hygrometer to ſtand at XCVIII, at any given degree of heat, and that the heat afterwards increaſes 5° of Reaumur, and then that the hygrometer ſtands at LXXXIV and four-tenths, and it is required to know to what cauſe this aſcent is to be attributed.

Remark, in the firſt obſervation, at what thermometrical diſtance XCVIII ſtands from C, that is, what degree of *cold* or decreaſe of heat it wants to bring it to C. Now in the ſecond column of this table you will find 1°,399 oppoſite to, and correſponding with XCVIII, therefore it wants 1°,399 degrees of Reaumur to bring it to C. But if the heat be increaſed five degrees more, then it is plain it will want 6,399 degrees of cold to bring it to C. Look then in the ſecond column of the table for this number; you will not find it exactly, but you will find it is intermediate between the tabular numbers 6,182 and 6,561, but nearer

to this laſt, conſequently nearly correſponding with the hygrometrical degree LXXXIV, the degree found in the ſecond obſervation; therefore you may conclude, that this degree of *apparent* dryneſs did not proceed from any *decreaſe* of vapours in the air, but ſolely from the *increaſe* of heat.

But, 2do. If in the ſecond obſervation the hygrometer had been at LXXXVI, then it is plain that it ſtands about two hygrometrical degrees higher than five degrees of heat would have brought it to, and conſequently that there was an *increaſe* of as much vapour as would raiſe the hygrometer about two degrees.

3tio. If, in the ſecond obſervation, the hygrometer had been found at LXXX, it is plain that this is four hygrometrical degrees lower than the acceſſion of five degrees of heat would have ſunk it to; therefore there was a decreaſe of as much vapour as would lower the hygrometer four degrees if the firſt temperature had remained.

Again, to diſcover the effects of cold and diſtinguiſh them from the effects of a *real acceſſion of vapour* the ſame caſe will ſerve, only inſerting the obſervations, (for 1°,399 degrees of cold would bring XCVIII to C.) Suppoſe then the firſt obſervation to be LXXXIV nearly, and that the air being cooled five degrees of Reaumur,

mur, the hygrometer at the ſecond obſervation ſtands nearly at XCVIII. Oppoſite LXXXIV, I find 6°,561 in the ſecond column, which denotes that 6,561 degrees of cold are required to produce C, but of theſe 6, &c. 5 have actually accrued; therefore only 6,561 — 5 = 1,561 remain; and in the ſecond column I find this number intermediate between 1,961 and 1,399, but nearer to this laſt; therefore the effect produced is ſolely attributable to cold and not to any new increaſe of vapour.—If after the acceſſion of five degrees of cold the hygrometer ſtood at XCIX, then it would follow that as much vapour had been produced as raiſed the hygrometer one degree.—If, on the contrary, on the ſecond obſervation it were found, that, after the acceſſion of five degrees of cold, the hygrometer ſtood at only XC degrees, it would prove that there had been ſuch a *diminution* of vapour as counteracted the influence of cold, and would, at the temperature of the firſt obſervation, make it ſtand eight hygrometrical degrees lower than at firſt.

The uſe of this table with reſpect to obſervations made at different *places*, in different temperatures, and preſenting different hygrometrical degrees, is to find by it in which of thoſe places the real quantity of moiſture is greateſt. This is found by reducing the temperature at one of thoſe places to that of the other, ſo that the effect of mere temperature may be excluded, that thus we may

may ſee what hygrometrical degree the obſervation ſo reduced would exhibit, whether higher, lower, or equal to the other.

Thus, ſuppoſing an hygrometer on a mountain, at the temperature of 7° Reaumur, to ſtand at XCVI, and an hygrometer on a plain, at the temperature of 15° Reaumur, to ſtand at LXXX, and it is required to know at which place moſt vapour exiſts in the air, let us then reduce the hygrometer on the plain to the temperature of that on the mountain, and ſee at what hygrometrical degree, being ſo lowered, it would ſtand by finding its diſtance from C, and to reduce it to 7°, the temperature of the mountain, I muſt take away 8° of heat, for 15° — 8° = 7°, and conſequently ſuperinduce 8° of cold. Now I find by this third table, that an hygrometer at LXXX, let its temperature be what it may, wants but 8°,151 degrees of cold to bring it to C, therefore, by thus ſubſtracting eight degrees of cold, I find it will arrive very nearly at C, for it wants only 0,151 of it; therefore more vapour exiſts on the plain than on the mountain, ſince the hygrometer on the mountain at the ſame temperature marks only XCVI.

This problem may be reſolved *more readily* by inſpecting the firſt table; for there we find that XCVI, at the temperature of 7° of Reaumur, indicate about 7,6 grains of moiſture in the cubic foot, and that LXXX, at 15° Reaumur, indicate 8,045 grains—

or

or ſtill more exactly thus: the hygrometer on the mountain being heated eight degrees more (that is to 15°) will ſtand at nearly LXXV, for it is already heated to 7°, and by the third table wants 2°,373 degrees of cold to bring it to C; but when heated 8° more, it will require 10,373 degrees of cold to bring it to C, and, conſequently, will mark nearly LXXV. Now, by the firſt table, LXXV at 15° Reaumur denote 7,205 of moiſture, and LXXX at 15° denote 8,045.

Note—I regret much the want of Mr. Leſley's hygrometer (an inſtrument as ſimple as it is ingenious) to compare its indications with thoſe of Sauſſure's. See 3d vol. Nicholſon's Jour. 461.

Synoptical View of the Stat

By RICHARD

1801	BAROMETER. Highest.	Day it happened.	Lowest.	Day it happened.	Mean of the Month.
January	30,26	7th to 11th incl. W. var. E. to S. and W.	29,14	4th, S.	29,94
February	30,42	7th to 10th incl. var. N. W. and E.	29,00	22d, W.	29,89
March	30,68	4th, W.	28,95	21st, W.	29,97
April	30,76	13th, N.	29,45	7th, W.	30,28
May	30,46	6th, E.	29,68	22d, S.	30,03
June	30,63	7th, 8th and 9th, W.	30,02	2d, W.	30,31
July	30,50	21st and 23d, W. N.	29,50	8th, S. E.	29,97
August	30,60	7th, W.	29,90	31st, W.	30,32
September	30,57	15th, var. S. W. E.	29,54	4th, E.	30,12
October	30,60	26th, S W.	29,10	18th, N.	29,99
November	30,47	5th, N W.	29,12	30th, E.	29,94
December	30,46	31st, W. to N.	28,80	26th, N.	29,63
Mean of the Year	30,534		29,350		30,032

Synoptical View of the State of the Weather at Dublin in the Year 1801.

By RICHARD KIRWAN, *Esq; L.L.D. F.R.S. P.R.I.A. &c.*

1801	BAROMETER. Highest.	Day it happened.	Lowest	Day it happened.	Mean of the Month.	THERMOMETER. Highest in the Day.	Lowest at Night.	Mean of the Month	RAIN. Days.	Inches.	STORMS. No.	Winds.
January	30,26	7th to 11th incl. W. var. E. to S. and W.	29,14	4th, S.	29,94	51,	28,	40,980	19. On 2 of which fell snow	1,261112	6.	W. by S. S. and W.
February	30,42	7th to 10th incl. var. N. W. and E.	29,00	22d, W.	29,89	56,5	29,	41,675	17. On 2 of which fell snow.	1,223701	4.	S W. W. and W. by N.
March	30,68	4th, W.	28,95	21st, W.	29,97	61,	25,	44,590	16. On 4 of which fell a little snow	1,169162	3.	S. W. and S W.
April	30,76	13th, N.	29,45	7th, W.	30,28	62,5	27,	45,205	9. On 4 of which fell hail and snow.	0,867014	3.	W. S W. and N.
May	30,46	6th, E.	29,68	22d, S.	30,03	63,	38,	53,485	16	1,969985	1.	S.
June	30,63	7th, 8th and 9th, W.	30,02	2d, W.	30,31	74,	37,5	57,785	7	0,722505		
July	30,50	21st and 23d, W. N.	29,50	8th, S. E.	29,97	75,	44,	59,480	19	4,019794	Thunder on 2 Days.	
August	30,60	7th, W.	29,90	31st, W.	30,32	73,	51,	61,885				
September	30,57	15th, var. S. W. E.	29,54	4th, E.	30,12	70,	43,	58,055	18	2,679863	Lightning 1 Night.	
October	30,60	26th, S W.	29,10	18th, N.	29,99	67,	34,	50,535	18. On 2 of which some hail fell	2,850632	2.	W. and S W. Thunder and Lightning.
November	30,47	5th, N W.	29,12	30th, E.	29,94	58,	25,	41,815	17. On 1 of which a little snow.	3,468058	2.	S E. and N.
December	30,46	31st, W. to N.	28,80	26th, N.	29,63	50,	23,	35,846	19. On 2 of which a little snow and 1 some hail.	1,734029	1.	W.
Mean of the Year	30,534		29,350		30,032			49,278	175,	21,965855	22.	Total in the Year

Fig. 1.

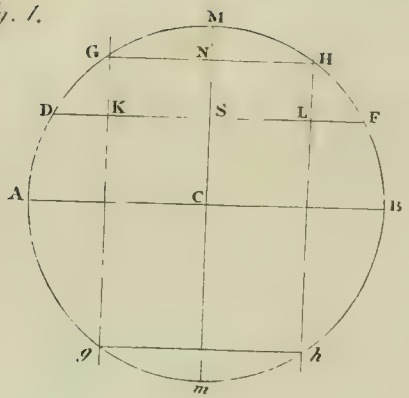
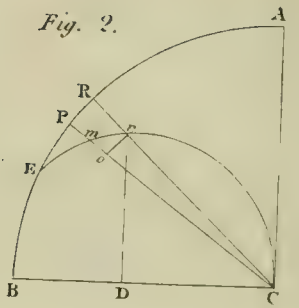


Fig. 2.



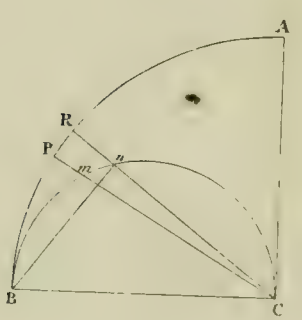


Fig. 3.

On determining INNUMERABLE PORTIONS *of a* SPHERE, *the* SOLIDITIES *and* SPHERICAL SUPERFICIES *of which* PORTIONS *are at the ſame Time algebraically aſſignable. By the Rev.* J. BRINKLEY *A. M. ANDREWS Profeſſor of Aſtronomy in the Univerſity of Dublin.*

Read, Nov. 2, 1801.

IN the year 1692, Viviani, a Florentine, who had been the pupil of Galileo, propoſed to the mathematicians of the age a problem, in which it was required to aſſign a portion of the ſurface of a ſphere exactly quadrible. This problem, truly indeterminate, and known by the name of the Florentine problem, was readily ſolved by ſeveral in various ways*. The conſtruction, however, of Viviani, which was afterwards publiſhed by himſelf, without demonſtration, exceeded the others in ſimplicity and elegance. It cannot now be known in what manner Viviani

* Wallis's Works, vol. ii. p. 478, and the Leipſic Acts, 1692.

Viviani demonſtrated his conſtruction. It may be readily and accurately done by methods familiar to geometricians before the diſcovery of fluxions and of the differential calculus. Yet Viviani ſays of the problem "Cujus divinatio, a ſecretis artibus "illuſtrium analyſtarum vigentis ævi, expectatur, quod in geometriæ "puræ hiſtoria tantummodo verſatus ad tam recondita videatur inva-"lidus." By "ſecretæ artes" there can be no doubt he deſigned the method of fluxions and the differential calculus (at that time in manner concealed by Newton and Leibnitz); it therefore may be ſuppoſed that Viviani's demonſtration was not the ſimpleſt one of which the conſtruction of his problem admits. Undoubtedly the demonſtration, independent of fluxions, is much leſs difficult than the problems propoſed by Paſcal concerning the cycloidal ſolids, in which the mathematicians of the middle of the ſeventeenth century were ſo much engaged. Euler long afterwards propoſed to determine a portion of a ſphere, which inſtead of its ſurface ſhould have its ſolidity accurately aſſignable. Euler has given a very ingenious ſolution of this indeterminate problem, but not one that merits the epithet of elegant, which he juſtly gives to the problem. His ſolution, being derived from the method of coordinates and double integrals, is much leſs ſimple than

than what a conſideration of the properties of the ſphere would have furniſhed. By proceeding upon too general principles the eaſieſt caſe eſcaped him. This caſe has lately been diſcovered and announced in the ſecond mathematical volume of the French National Inſtitute, by M. Boſſut, from which it appears that Viviani's conſtruction of the Florentine problem determines at the ſame time a portion of the ſurface of the ſphere, and a portion of its ſolidity, both exactly aſſignable. M. Boſſut tells us, that the analyſis of his ſolution is much more difficult than that of Viviani's conſtruction, and promiſes to communicate it hereafter. In the mean time, Mr. Woodhouſe has given a very ingenious demonſtration of it in the firſt part of the Phil. Tranſ. for 1801. This demonſtration, however, being performed by the method of coordinates and double integrals, is leſs ſimple than the problem admits.

What ſeems accidental in Viviani's conſtruction will take place in an innumerable variety of inſtances; and it is the object of the preſent Eſſay to ſhew that "*there are innumerable conſtructions by which portions of a ſphere may be obtained ſo that the ſpherical ſuperficies and ſolidity of each portion are accurately aſſignable.*

Lemma.

If a ſphere be perforated by a right cylinder, the axis of which paſſes through the centre of the ſphere, the ſolid content of the remainder of the ſphere is to $\frac{1}{6}$ of a cube, the ſide of which is the length of the cylinder entirely within the ſphere, as the circumference of a circle to its diameter.

Dem. Let A M B *m* be a ſection of the ſphere through the axis of the cylinder M *m*. Let G M H *h m g* be a ſection of the cylinder, $Gg = Hh$ its length wholly within the ſphere, and let A B and D F be drawn parallel to G H, *g h* which will therefore be diameters of ſections of the ſphere perpendicular to M *m*.

Fig. 1.

The area of the ſection through D F : ſect of cylind. :: $DS^2 : KS^2$ therefore dividendo

ſect. of remainder : ſect. of cylind. :: $DS^2 - KS^2 : KS^2$

but ſect. of cylind. : great circle :: $KS^2 : AC^2$

whence

Sect.

ſect. of remainder : great circle :: $DS^2 - KS^2$ (GN^2) = $CN^2 - CS^2$: AC^2 ; or ſect of remaind. : $CN^2 - CS^2$ :: great circle : AC^2 : circumf. : diameter.

Now if on the ſquare on CN as a baſe, a cube and pyramid of the ſame height be conſtructed, the difference of the areas of any ſections of the cube and pyramid at the diſtance CS from the vertex $= CN^2 - CS^2$. And the ſolids of equal heights, whoſe ſections at the ſame height are always in the ſame given ratio, are in the ratio of their baſes, therefore the remainder of hemiſphere : diff. between the cube and pyramid :: circumference : diameter. But the difference between the cube and pyramid $= \frac{2}{3}$ of the cube. Hence the remainder of the hemiſphere : $\frac{1}{3}$ cube on CN :: circumference : diameter, or remainder of ſphere : $\frac{1}{3}$ cube on CN $= \frac{1}{6}$ cube on Gg :: circumference : diameter. QED.

PROBLEM. ABC being a quadrant of a great circle of a ſphere, the centre of which is C, it is required to find the nature of the curve E*mn*C, ſo that the ſurface of the ſphere inſiſting perpendicularly over the area EA*nm*C may be algebraically aſſignable; and alſo that the ſolidity inſiſting perpendicularly on the ſame area may be algebraically aſſignable. Fig. 2.

SOLUTION.

Solution. Let the rad. $A C = r$, $C n = z$, the meaſure of the L A C R to rad. unity $= a$, and let $o n$ be a circular arc deſcribed with radius $C n$ (z). Becauſe the ſurface of a zone contained between a great circle and a parallel circle equals the rectangle contained by the circumference of the great circle, and the perpendicular diſtance between the circles*; it readily follows that the ſurface of the ſphere inſiſting perpendicularly above and below $P R \, n o = 2 P R \times \sqrt{C R^2 - C n^2} = 2 P R \times \sqrt{r^2 - z^2}$. This ſurface is to the ſurface ſtanding perpendicularly on $P m n R$ ultimately in the ratio of equality. Therefore the fluxion of the ſurface over $A C n R = 2 r \dot{a} \sqrt{r^2 - z^2}$. Again it eaſily follows from the lemma that the ſolid inſiſting perpendicularly on $P R \, n o = \frac{P R}{2 C R} \times \frac{1}{6}$ cube of the perp. at $n = \frac{2}{3} \frac{P R}{C R} \times \overline{r^2 - z^2}^{\frac{3}{2}}$, and the ratio of this ſolid to the ſolid inſiſting on $P R n m$ is ultimately the ratio of equality. Hence the fluxion of the ſolid inſiſting on $A C n R = \frac{2}{3} \dot{a} \overline{r^2 - z^2}^{\frac{3}{2}}$.

Fig. 2.

It follows therefore that the value of $\dot{a}$ muſt be ſuch that the fluents of $\left\{ \begin{matrix} 2 r \dot{a} \sqrt{r^2 - z^2} \\ \frac{2}{3} \dot{a} \times \overline{r^2 - z^2}^{\frac{3}{2}} \end{matrix} \right\}$ may be both algebraically expreſſed. This it readily appears can take place by aſſuming an infinite variety of values of $\dot{a}$ in terms of $\dot{z}$, z and conſt. quantities. The problem conſequently is truly unlimited.

To

* Archimedes de Sphæra & Cylindro.

To conſider more particularly ſome of theſe caſes,

1. Let $\dot{a} = \frac{\dot{z}}{\sqrt{r^2 - z^2}}$

and the fluent of $\begin{cases} 2 r \dot{a} \sqrt{r^2 - z^2} = \text{fl. } 2 r \dot{z} = 2 r z = (\text{when } z = r)\ 2 r^2 \\ \frac{3}{2} \dot{a} \times r^2 - z^2 \overline{)^{\frac{3}{2}}} = \text{fl. } \frac{3}{2} \dot{z} \times r^2 - z^2 = \frac{3}{2} r^2 z - \frac{3}{5} z^3 = (\text{when } z = r) \frac{4}{5} r^3. \end{cases}$

Becauſe in this hypotheſis $\dot{a} = \frac{\dot{z}}{\sqrt{r^2 - z^2}}$ or $\dot{a}\ r = \frac{r\ \dot{z}}{\sqrt{r^2 - z^2}}$ Fig. 3.

therefore $a\ r$ = circ. arc rad. r and ſine z, or s, A R = C n, or coſine of B R = C n. Conſequently the angle B n C is a right angle, and the locus of n or the curve C n m is a ſemicircle deſcribed on the diam. B C. This caſe therefore furniſhes the ſolution which Viviani himſelf gave of the Florentine problem.

If on the radius which is the continuation of B C another ſemicircle be deſcribed and the ſame conſtruction made, the ſurface of the hemiſphere inſiſting perpendicularly between the ſemicircle and the quadrants = $2 \times 2\ r^2$ = the ſquare of the diameter of the ſphere.

Also, if a circle be deſcribed on each radius of a diameter of a ſphere, and the ſphere be perforated by cylinders inſiſting perpendicularly

dicularly on theſe circles, the remainder of the ſphere $= 4 \times \frac{4}{9} r3 = \frac{2}{9}$ cube on the diameter of the ſphere, as M. Boſſut has announced in the ſecond volume of the French National Inſtitute*.

2. If $\dot{a} = \frac{m \dot{z} z}{r^2}$ (m being a numerical coefficient) then the correct fluents of $\begin{cases} 2 r \dot{a} \times \overline{r^2 - z^2}^{\frac{1}{2}} = \frac{1}{3} m \times r^2 - \frac{\overline{r^2 - z^2}^{\frac{3}{2}}}{r} \\ \frac{2}{3} \dot{a} \times \overline{r^2 - z^2}^{\frac{3}{2}} = \frac{2}{15} m \times r3 - \frac{\overline{r^2 - z^2}^{\frac{5}{2}}}{r^2} \end{cases}$

when $z = r$ the ſurface becomes $\frac{2}{3} m r^2$ and the ſolid $\frac{2}{15} m r3$. The curve in this caſe ſatisfying the problem cannot paſs through B, for by taking the fluents $a = \frac{m z^2}{2 r^2}$, and therefore if the curve paſſed through B, $m = 2 a =$ ſemi-circumference of a circle, the rad. of which is unity, and not an aſſignable number, conſequently the ſolid and ſurface could not be algebraically expreſſed.

Fig. 2.

If

*By help of the theorem of Archimedes for the ſurface of a ſpherical zone Viviani's concluſion may, without difficulty, be accurately deduced by method known to geometricians before the invention of fluxions and by the help of the lemma above given Boſſut's theorem might alſo be deduced by the ſame method, independent of fluxions. Hence Viviani's aſſertion reſpecting the difficulty of ſolving the problem from geometrical conſideration alone, cannot be conſidered as juſt.

If m be taken, any number having a leſs ratio to unity than the circumference of a circle to its diameter the curve will cut AB. If m be taken, having a greater ratio to unity than the circumference to the diameter, the curve will cut the continuation of AB, and in both caſes the conditions of the problem will be anſwered.

If $\dot{a} = \frac{m \dot{z}^{n} z}{r^{n+1}}$, n being any odd number and m a numerical coefficient

the fluents of $\begin{cases} 2 r \dot{a} \sqrt{r^2 - z^2} = \text{fl.}\ \frac{2 m \dot{z}^{n} z}{r^n} \sqrt{r^2 - z^2} \\ \frac{2}{3} \dot{a} \times \sqrt{r^2 - z^2}^{\frac{3}{2}} = \text{fl.}\ \frac{2}{3} m \dot{z}^{n} z \times \sqrt{r^2 - z^2}^{\frac{3}{2}} \end{cases}$

will be had in finite terms, and therefore the conditions of the problem will be anſwered. From the equation $a = \frac{m z^{n+1}}{n+1 \, r^{n+1}}$ the value of m may be determined ſo that the curve may cut AB

or its continuation. Becauſe $\dot{a} = \frac{m z \dot{z}^{n}}{r^{n+1}}$, if p = the perpendicular from C on the tangent to the curve, $\frac{m z^{n+1} \dot{z}}{r^{n+1}} : \dot{z} :: p : \sqrt{z^2 - p^2}$. Therefore $r^{2n+2} p^2 = m^2 z^{2n+4} - m^2 z^{2n+2} p^2$ or $p = \frac{m z^{n+2}}{\sqrt{r^{2n+2} + m^2 z^{2n+2}}}$, the equation of the ſpiral of which E n C is a portion.

3. Let $\dot{a} = \frac{m z \dot{z}^{n}}{r^n \sqrt{r^2 - z^2}}$ n being any whole poſitive number, and m a numerical coefficient,

the fluent of $\left\{ \begin{array}{l} 2 r \dot{a} \sqrt{r^2 - z^2} = \frac{2 m r^2}{n+1} \\ \frac{2}{3} \dot{a} \times \overline{r^2 - z^2}^{\frac{3}{2}} = \frac{4 m r^3}{3 n+1 . n+3} \end{array} \right\}$ when $z = r$.

Becauſe in this hypotheſis $\dot{a} = \frac{m z \dot{z}^{n}}{r^n \sqrt{r^2 - z^2}}$, when n is an odd number the relation of a and z will be algebraical; when n is

n is even the fluent of $\frac{m z^{n} \dot{z}}{r^{n}\sqrt{r^2 - z^2}}$ can be found by help of the circle.

Therefore if the value m be taken according to the different values of n ſo that z may become equal to r, innumerable ſolutions will be had.

By theſe and other obvious methods innumerable ſolutions may be obtained. Curves may be obtained that ſhall paſs through neither B nor C, or through B and not through C.

The curves above obtained are, except the circle, ſpirals.

4. If it be required that the curves be ſuch that the relation between the abſciſſa C D $= x$, and the ordinate D $n = y$ may be algebraical; ſuch curves may be found, but not ſo readily as the above. When the curve is algebraical z is an algebraical function of x and y, whence, becauſe $z = \frac{x}{s, a}$ and $z = \frac{y}{cs, a}$, it follows that s, a and cs, a are algebraical functions of z. Fig. 2.

1. Let $s, a = \frac{z}{r}$, then $cs, a = \sqrt{1 - \frac{z^2}{r^2}}$

therefore $\dot{a} = \frac{\dot{z}}{r} \times \frac{1}{\sqrt{1 - \frac{z^2}{r^2}}} = \frac{\dot{z}}{\sqrt{r^2 - z^2}}$

 This

This value of $\dot{a}$, as was ſhewn above, anſwers the conditions of the problem.

Now $x = z\,s$, $a = \frac{z^2}{r}$, $y = z\,cs$, $a = \sqrt{z^2 - \frac{z^4}{r^2}}$

whence $y^2 = z^2 - \frac{z^4}{r^2} = r\,x - x^2$, which is the equat. to the circle, as before found.

2. Let s, $a = \frac{z^2}{r^2}$, then cs, $a = \sqrt{1 - \frac{z^4}{r^4}}$

and $\dot{a} = \frac{2\,z\,\dot{z}}{r^2\sqrt{1 - \frac{z^4}{r^4}}} = \frac{2\,z\,\dot{z}}{\sqrt{r^4 - z^4}}$

Hence $2\,r\,\dot{a}\sqrt{r^2 - z^2} = \frac{4\,z\,\dot{z}}{\sqrt{r^2 + z^2}}$

$$\frac{2}{3}\,\dot{a} \times \overline{r^2 - z^2}^{\frac{3}{2}} = \frac{4\,z\,\dot{z}}{\sqrt{r^2 - z^2}} \times \sqrt{r^2 - z^2}$$

consequently the fluent of $\begin{cases} 2\,r\,\dot{a}\sqrt{r^2 - z^2} = \sqrt{2 - 1} \times 4\,r^2 \\ \frac{2}{3}\,\dot{a} \times \overline{r^2 - z^2}^{\frac{3}{2}} = \frac{4}{9}\,r3 \times 4\sqrt{2} - 5 \end{cases}$ when $z = r$

and

and therefore in this caſe the ſuperficies and ſolid are algebraically aſſignable.

Becauſe $x = z\,s$, $a = \frac{z^3}{r^2}$, and $y = z\,c\,s$, $a = \sqrt{z^2 - \frac{z^6}{r^4}}$ it follows that

$x^2\ r^4 = \overline{y^2 + x^2}\,|^3$ is the equation to the curve in this caſe. This curve will paſs through B, becauſe when $z = r$, s, $a =$ radius.

POLITE LITERATURE.

On the CHOICE *of* SUBJECTS *for* TRAGEDY.

By WILLIAM PRESTON, *Esq; M.R.I.A.*

Read, Jan. 11th, 1800.

I PERCEIVE by the tendency of a late queſtion, which the Academy has thought fit to propound, on the ſubject of *German* literature, that ſome among you are not diſpoſed to bow with idolatrous humility before the golden calf of commonly received opinion. I ſhall therefore, without further preface, venture to lay before the Committee of Polite Literature, whom I beg leave to conſider as the natural guardians of the infant taſte and undirected and unſupported genius of this country, a few brief and haſty obſervations on a critical remark, which, as it appears in a literary journal of ſome credit and reſpectability, may paſs unqueſtioned by many readers. An implicit deference to the arbitrary ſentence of ſelf-erected authority is as incompatible with the intereſts of pure taſte, as it is ſubverſive of the ſpirit of true religion and true morality.

THE obſervation to which I allude is to this effect.—"It has commonly been thought a judicious rule with reſpect "to tragedy, not to borrow the ſtory from recent events, "nor has any writer within our recollection tranſgreſſed this "law of limitation with impunity. Among other obvious "reaſons for the rule it may be obſerved, that it converts the "ſtage from an inſtrument of amuſement into a field of poli-"tical altercation, and tends to intereſt the paſſions on the "ſide of the popular judgment on any paſſing event, before "diligent enquiry and cool diſcuſſion can have determined "whether that judgment be conſonant to truth."

I MUST obſerve in the outſet that the authors of this remark, in conſidering the ſtage only as a mere inſtrument of amuſement, ſeem to think too meanly of its functions and character. *Shakeſpeare* had juſter and higher notions of the dignity and importance of the ſtage, when he ſaid, "Let the "players be well uſed, they are the abſtract and brief chro-"nicles of the time.—Its end," ſpeaking of the ſtage, "both at "the firſt and now, was and is to hold as 'twere the mirror "up to nature, to ſhow virtue her own features, ſcorn her "own image, and the very age and body of the time his "form and preſſure."

IN fact, thoſe who adopt the language and ſentiments of the remark which I have ſtated, ſeem to depend too much on the

the *green room* for their notions of dramatic compoſition, and to conſider rather what is likely to gain the ſanction of the Lord Chamberlains, or to paſs the ordeal of ſtage-repreſentation, than what is really eſtimable in itſelf, or conformable to the true ends of dramatic poetry, Obvious reaſons there may be of a prudential nature, and merely perſonal to the writers, which may deter the timid and cautious poet, whoſe thoughts are wholly bent on preſent emolument, from chuſing ſuch ſubjects as he conceives to be *periculoſæ plenum opus aleæ*, or which he ſuppoſes may incur the danger of being ſmothered at the threſhold of their paſſage into ſtage-exiſtence by the cautious delicacy of a licenſer, laudably awake and ſore to every thing like an alluſion to modern politics, or expoſed to the reſentment and malicious exertions of party, employed to procure the condemnation of a play for the political ſins of its author. Such prudential motives as theſe are not general principles of ſound criticiſm; theſe principles can only be drawn from the prevailing character and deſtination of dramatic poetry, as they are explained and taught by the beſt criticks antient and modern, and as they may be traced out in the writings of the great maſters of dramatic poetry.

The preſent ſtate of the *Engliſh* ſtage ſhews that the public taſte has undergone a great and by no means favourable revolution,

volution, and the ephemeral productions, which ſucceſſively appear in its preſent hour of degradation, afford melancholy proofs that the favourable reception of the public is now no longer an unequivocal teſt of intrinſic merit. The prevailing rage for the immoral and extravagant productions of the *German* ſchool puts this aſſertion paſt all diſpute.

There is, indeed, one objection to the choice of ſubjects too near the preſent time, which has conſiderable weight, namely, that, in general, fiction muſt be employed for the purpoſe of fitting ſubjects taken from real life for dramatic repreſentation, and the mind naturally revolts againſt the uſe of fiction where the tranſactions are ſo recent that the public mind is in poſſeſſion of all their circumſtances; but if the events ſhould happen in themſelves to be ſo grand, ſo affecting, ſo full of truly intereſting incidents, as to furniſh a tragic fable ſufficiently detailed and diverſified, without the neceſſity of reſorting to invented circumſtances of embelliſhment or intereſt, the obſervation, *incredulus odi* no longer applies, and our knowledge of the reality of the tranſaction, and the newneſs and freſhneſs of the concomitant emotions, which it has already excited, will encreaſe the intereſt and pathos.

The Greek tragic writers, if their example has any weight, were fond of compoſing tragedies on political ſubjects. A reference

ference to queſtions of national importance, and topics that employed the public mind, was conſidered as an excellence, and the more directly a play was brought to bear on the preſent times, the more happy was the poet eſteemed in his choice of a ſubject. The *Perſæ* of *Æſchylus* was written by him and repreſented at *Athens* no long time after the battle of *Salamis*, which it celebrates, and at which the author was preſent. The *Supplices*, the *Heraclidæ*, and the *Oedipus Coloneus* were expreſsly written for the purpoſe of paying a compliment to the *Athenian* people. The conteſt for ſovereignty between the Greeks and Aſiatics was a topic, of which the poets of the former people, from *Homer* downwards, never loſt ſight. Among the tragedies of *Seneca* there is one entitled *Octavia*, of which the unfortunate daughter of *Meſſalina* and wife of *Nero* is the heroine. *Shakeſpeare*, who was a ſtranger to the rules of falſe criticiſm, and conſulted only nature and good ſenſe, did not find any thing in the recent date of a tranſaction which rendered it leſs proper, in his opinion, for becoming the ſubject of dramatic repreſentation. Many of his moſt popular plays are formed on the tranſactions of *Engliſh* hiſtory, particularly the conflicts between the houſes of *York* and *Lancaſter*, which were then freſh in the mind of every body. The tragedy of *Henry* the *eighth*, in which the ſovereign of *England* is a principal perſonage, and the divorce and death of his royal conſort are introduced, was brought on

the

the ſtage very near the time when the events happened, and even while the daughter and ſucceſſor of that monarch was on the throne. The lines in the prophetic ſpeech of *Cranmer*, which are meant to convey a compliment to *James* the *firſt*, were inſerted after the death of *Elizabeth*, as appears from the aukward manner in which they are introduced and connected with the foregoing and ſubſequent lines. In the play of the *Midſummer Night's Dream*, there are lines, which in the opinion of the commentators, refer in a particular manner to the politics of the day:

> ——————— I ſat on a promontory
> And heard a mermaid on a *dolphin*'s back
> Uttering ſuch dulcet and harmonious breath,
> That the rude ſea grew civil at her ſong,
> And certain ſtars ſhot madly from their ſpheres
> To hear the ſea-maid's muſic.* &c. &c.

We find *Dryden* adapting the ſtory of the cruelties practiſed at *Amboyna* to the ſtage, at the time when the *Britiſh* court had determined on a rupture with the United States of *Holland*. *Rowe*'s fine play of *Tamerlane* was expreſsly written with a reference to the politics of the day, and under the perſonages of *Tamerlane* and the ferocious Turk it is well known, that the poet meant to exhibit the portraits of *William* the *third* and his ambitious rival and opponent *Lewis* the *fourteenth*. Even the

* See Dr. Warburton's notes on the whole paſſage.

the French ſtage, which has ever been conſidered as a model of guarded propriety, is not without examples of plays, written with a view to modern politics, or, at leaſt, with a deſign of influencing the public opinion. Even the correct, the decorous and modeſt *Racine* has done this, in his tragedy of *Eſther*. The *Mahomet* of *Voltaire* was compoſed with a deſign of diſcrediting fanatical enthuſiaſm, and hypocritical impoſition, on the part of the miniſters of religion, and ſuperſtitious and ferocious bigotry among their adherents. Theſe ideas are purſued in the *Alzire*, and ſimilar topics are diſplayed and enforced, by a writer of the preſent day, in the Tragedy of *Carl neuf ou L'Ecole des Rois*.

Recent events and modern politics may be introduced on the Stage in three different Manners.—Modern events may be ſelected as the ſubjects of the Drama—Stories may be taken from ancient hiſtory and adapted, on the ſpur of the occaſion, for the expreſs purpoſe of preſenting parallels to recent events, or affording alluſions to modern politics; or, laſtly, occaſion is taken, to introduce reflections of that nature in particular paſſages: and of theſe different forms of reference the beſt writers ancient and modern, as I have ſaid, afford us examples.

But the juſtification of dramatic ſubjects, which have a political aſpect, does not reſt merely on example; they may be defended on principle. I will not go ſo far as to aſſert, with ſome critics, that the chief end of poetry is to inſtruct;—that *Homer*, for in-

ſtance, wrote his *Iliad*, on purpoſe, to teach mankind the miſchiefs of diſcord among princes; and his *Odyſſey* to prove to them the advantages of ſtaying at home and taking care of their families.—If, diſmiſſing theſe lofty notions of the end of tragedy, we admit with *Ariſtotle*, that its object and deſtination are only to afford that pleaſure which reſults from fictitious terror and pity—*την απο ελεε και φοβε δια μιμησεως ηδονην*,—in this point of view, the recent date of the tranſactions, the freſhneſs of the impreſſions they have excited, the conſciouſneſs of the reality of the incidents, of the diſtreſs and calamity of the ſufferers, for inſtance, will encreaſe the degree of pity and terror excited by the Drama, as well as the curioſity and intereſt of the ſpectators; and thus will augment the power in the ſtage of communicating pleaſure.

Yet, though I may be ready to allow, that utility and inſtruction are not the prime or immediate end of poetry; few will deny, that it may and ought to be rendered uſeful. It ſeems to be a certain narrowneſs of mind, which would debaſe poetry to the humble province, of merely amuſing a vacant hour. It ſeems to be the general opinion of critics, that every perfect Drama ought to have a particular moral, and that by how much the more important and comprehenſive this moral, by ſo much the greater is its excellence. Now, what are politics, truly underſtood, but morality enobled and extended—the morality of

of communities and ſtates?—The public morality differs from the private only in ſuperior comprehenſiveneſs, utility and dignity. The political, therefore, is the more excellent moral, and the merit of inculcating it is greater, inaſmuch as the reformation of a whole people is more important than that of a ſingle private perſon. In my mind, therefore, we might as well reprobate a dramatic performance for conveying a moral, as for bearing an application to modern politics.

It is ſaid, that political Dramas "tend to intereſt the paſſions on the ſide of the popular judgment, on any paſſing event"—be it ſo—if the popular judgment be right, why ſhould not the paſſions be intereſted in its favour? and may not the paſſions, in the hands of a judicious and virtuous writer, become the means of rectifying the popular judgment? The conduct of human agents is ſeldom governed by pure, unimpaſſioned, abſtracted reaſoning; they act, and are acted on, through the medium of their paſſions; and the great care of the moraliſt and the legiſlator is to give a juſt and uſeful bias to the paſſions of men. We find, that even the Chriſtian religion, in its precepts as well as its ſanctions, applies not leſs to the paſſions, than to the reaſon of men. If the ſtage can be made ancillary to the great end of morality, it becomes a moſt important inſtrument in the hands of government. We find that, among the *Athenians*, the theatre engaged no ſmall ſhare of the public attention, anxiety and protection; it was conſidered in the moſt important light, and even reputed a part of the eſtabliſhment of the ſtate. The

 French

French government, ſince the revolution, has ſhown an equal care of the theatre. The ſtage is one among various avenues to the public mind; and on the ſame principles on which the Stage is interdicted from meddling with modern politics, the Preſs, in general, might be prohibited from entertaining ſuch topics; ſince their diſcuſſion may tend to intereſt the paſſions on the ſide of the popular judgment on any paſſing event. The genius of a free government requires, that every mode and form of addreſſing the public feelings, and enlightening the public mind, ſhould not only be permitted, but encouraged; provided they confine themſelves within the bounds of decorum and moderation. Indeed where an arbitrary *deſpotiſm* prevails, where the government is every thing, the people nothing, there popular opinion has no weight or place, and popular feeling muſt be extinguiſhed. In ſuch a ſtate of ſociety, the ſtage muſt indeed forbear to glance at the politics of the day, and content itſelf with being a mere inſtrument of amuſement.

What I have hitherto ſaid applies peculiarly to Tragedy, but the ſame reaſoning will hold good with regard to the choice of modern incidents and characters from real life, as ſubjects of diſplay for the comic Muſe. Tradition informs us, that ſuch has been the practice of the beſt writers for the comic ſtage; and it is obvious, that the pleaſure reſulting from the drama will be heightened by our knowledge of the perſonages deſignated, and the conſciouſneſs that the portraits which it exhibits are not only

only well painted, but alſo juſt likeneſſes. We experience an additional and peculiar pleaſure, reſulting from our compariſon of the copy with the original, while we trace out the features of reſemblance. It may be ſaid, that ſuch a deſignation of perſons will give pain to the individuals, who are ſingled out and expoſed to public notice;—I reply, that in moſt caſes it is ſo much the better—it is not often practicable to make the virtuous and the reſpectable ridiculous—people can only become the object of contempt and laughter through their own follies, extravagancies and vices—the fooliſh and the vicious invite and deſerve correction; and the comic Muſe is in her right place and function, when ſhe inflicts it with the rod of ſatire. She is then aſſiſting the magiſtrate in the reformation of the public morals, by the chaſtiſement of offenders againſt virtue, good order and decorum.

REFLECTIONS *on the* PECULIARITIES *of* STYLE *and* MANNER *in the late* GERMAN WRITERS *whose* WORKS *have appeared in* ENGLISH, *and on the* TENDENCY *of their* PRODUCTIONS. *By* WILLIAM PRESTON, *Esq; M.R.I.A.*

Verbera, carnifices, robur, pix, lamina, tædæ.

Read, Nov. 24, 1800.

AN extraordinary revolution ſeems to be taking place in the republic of letters, as well as in other ſtates; and the muſes, in the more ſouthern parts of Europe, appear to be menaced with ſubjection, if not with extirpation, by invading ſwarms from the northern hive. That *England* is peculiarly in danger of this fate, appears, from the extraordinary degree of avidity, and almoſt excluſive attention, with which the public has of late received every coruſcation of fancy from the north, however pale and lurid, however deficient in ſteady light and permanent ardour. It may not be an unentertaining or uſeleſs enquiry, to inveſtigate the pretenſions of ſome of the productions of the *German* muſe, which have lately appeared in an Engliſh dreſs.—If the admiration beſtowed on

on them is well founded, criticiſm will be profitably employed, in tracing the appropriate and ſtriking excellencies of theſe compoſitions, and recommending them as objects of ſtudy and imitation. If, on the other hand, the exceſſive predilection, indeed I may ſay the rage, which at preſent attends theſe exotic novelties, is capricious and unfounded, *they* will render a ſervice to the intereſt of true taſte and ſound morality, which muſt ever be inſeparable, who endeavour to oppoſe mounds to the prevailing torrent; who take up arms to reſiſt the irruption of *Gothic* barbariſm and ferocity. At all events, it muſt be amuſing to explore the grounds and cauſes of a literary phenomenon, ſo ſudden and ſo ſingular, as the general predominance of this new ſchool of compoſition.

I SHALL not attempt an accurate examination, in detail, of the numerous *German* productions, which have been introduced to the acquaintance of the *Engliſh* reader, through the medium of tranſlations. The due execution of ſuch a taſk would require a knowledge of the *German* language, which I want, and more labour and attention than I can ſpare; and would betray the writer into a prolixity of diſcuſſion far exceeding the compaſs of an eſſay and the meaſure of your patience. I ſhall reſt in generals, and content myſelf with a comprehenſive view of the ſtyle, the characteriſtics and tendency of thoſe writings which are ſo much the favourites of the paſſing hour, and promiſe to form an Epoch or rather Dynaſty in the annals of poetry.

I HAVE

I LEAVE to others the defence of the more modern *German* poets, and their *Engliſh* imitators and followers. I ſhall not, in diſcuſſing this topic, even affect the tone of doubt and enquiry, as if the queſtion of the merits or demerits of thoſe writers were ſtill open. Having drawn the articles of my poetical belief from *Ariſtotle*, and the critics ancient and modern, who have trod in his footſteps, I had learned to conſider the Greek tragedy as the moſt perfect model of dramatic compoſition; and I was confirmed in this perſuaſion, by obſerving, that thoſe modern writers, who formed their taſte by a ſtudy and imitation of the ancients, have approached neareſt to perfection. *Shakeſpeare*, indeed, forms an exception; but his ſtupendous and matchleſs abilities entitle their maſter to ſtand alone, and exclude any inference, as they exclude all compariſon. In general, regularity of plan, truth and conſiſtency of character, probability of incident, attention to decorum, propriety of ſentiment, chaſtity of diction, and correctneſs of compoſition, were commonly ſuppoſed to be of the very eſſence of good writing—but alas, *tempora mutantur et nos mutamur in illis*. I muſt own it has moved my bile *to mark the growth and prevalence of the ſtrange and prepoſterous* partiality for the Gothic productions of the German ſchool. The diſtempered rage for the gloomy, the horrible, the diſconnected, the diſproportioned and the improbable. As juſtly might we prefer the comfortleſs dark and extravagant ſtyle of the *Gothic* architecture; its ponderous maſſes, its elaborate littleneſs, and capricious profuſion of ornament, to the ſublime ſimplicity, the chaſte ſymmetry, and harmo-

nious integrity of the Greek and Roman models, as deſert the claſſic ſtyle of compoſition, to which the beſt modern writers have formed themſelves, by a diligent peruſal of the ſacred remains of antiquity, in favour of productions, of which it may be truly ſaid that they are to be peruſed with advantage, as exhibiting, by contraſt, the rules of good taſte, in ſpecimens of the reverſe of every thing that has uſually been ſuppoſed to form the life and ſoul of legitimate compoſition, in glaring examples of almoſt every fault into which a writer is liable to be ſeduced, by a puerile imagination, a depraved taſte, and a defective judgment. This revolution in literature is the more extraordinary at a time, when faſtidious delicacy and a liſtleſs ſlowneſs to be pleaſed are the characters of the age, and with exceſſive luxury, an uncommon effeminacy and ſoftneſs of manners are generally diffuſed. It is not leſs extraordinary, that amidſt the prevalent affectation of a prudiſh and ſanctimonious morality, and a more than ordinary attention, on the part of government, to correct and controul the licentiouſneſs of the preſs, writings of a tendency like that of the *German* Dramas and Novels ſhould be ſuffered to ſpread, and propagate their poiſon, without moleſtation or reproof from the Pulpit or the Police.

It will appear, on a peruſal of thoſe *German* writings of the neweſt ſtamp, which have been tranſlated into *Engliſh*, that the language is either bombaſtic and tumid, often unintelligible, or elſe, groſsly familiar and vulgar; the ſentiments forced, unnatural, and

and, too frequently, not only immoral but impious; the characters, extravagant caricatures, unlike any thing in nature, or if the reſemblances ever did or do exiſt, that the prototypes are to be found only in *Bedlam*. The incidents are as improbable, as the characters are monſtrous. They are introduced with little preparation, and thrown together, without order, and without connexion and mutual dependency. Meantime, by the courteſy of *England*, horror muſt rank as pathos, wildneſs of character and improbability of incident as originality of genius, bombaſt and rant as ſublimity, far-fetched and unnatural ſentiment as ſtrength of thought and boldneſs of conception.—If, indeed, to produce ſuch ſentiments as never were entertained or uttered by a rational being, if to exhibit ſuch perſonages as never yet appeared in real life, if to invert the uſual order of nature and art, and to explode all that common men conſider as ſacred and venerable, if to make a ſhambles and a charnel houſe of the ſtage—if all this be Originality, then it muſt be confeſt, that the writers of the *German* Romance and Drama are moſt truly original. But, leſt I ſhould be thought to deal in raſh and general cenſure, and to produce rather a prejudiced invective againſt the writings of this new ſchool, than a fair examination and eſtimate of their merits, let us proceed to conſider ſome of the more ſtriking peculiarities of the high Dutch Muſe, as we find them exemplified in the writings of *Gæthé*, *Schiller*, *Kotzebue* and *Burger*.

THE feature which will firſt draw the attention of every reader of the *German* Tragedy and Romance, is a ſombre gloom, a love of terror, an affectation of the horrible. Terror and pity are two of the moſt powerful emotions of the mind; and to excite them is the great object of tragedy, and of the tragic parts of the epos; but the attempt to out-herod Herod, to out-butcher butchery, and thus, by raiſing terror and pity in exceſs, to be ſomething more than pathetic, will ever defeat its own ends, and, in fact, ſeldom fails of producing ridicule. "*Ne* "*quid nimis*" is a maxim not leſs applicable to good writing, than to good conduct in the affairs of life. The ſtomach throws off over-large doſes of any medicaments, and thus the moſt potent drugs and even mortal poiſons may become inoperative. Nothing can be highly affecting, which is not, at the ſame time, highly probable; for it is the attribute of probability that brings the feigned action home to *men's buſineſs and boſoms*. Thoſe writers, who ſeek to terrify or melt, in a ſuperlative degree, outſtep the modeſty of nature, and wander out of the bounds of probability. This love of exhibiting the gloomy and horrible, may be compared to exceſs of Chiaro oſcuro in painting.* I know that ſome unreflecting readers are diſpoſed to give implicit credit to the love of horrors and the affectation of the wild and dreadful, as concomitants of ſtrong fancy and original genius: To ſuch I would recommend the peruſal of *Seneca* the tragedian, and of ſome of the earlier

* I ſhould not be ſurpriſed to ſee the executions of Ravaillac introduced on the ſtage by the Germans. Dryden's Amboyna is in their taſte.

earlier Italian dramatiſts, his diſciples, and imitators. Theſe moſt frigid and inſipid of all writers plunge into the deepeſt abyſs of horrors, and add to bombaſtic language and extravagant ſentiments an unexampled ferocity of ſpectacle; for *Seneca*, it muſt be known, together with his vapouring language, his holiday ſuit of fuſtian and buckram, bequeathed to his *Italian* followers his blood-ſtained mantle of cruelty. The hunters of literary rarities, who have ſwelled their libraries from Grub-ſtreet with all ſuch reading as was never read, will find the ſame love of horrors predominate in our worſt Engliſh tragedies, as, for inſtance, in the plays of *Banks*.* It is obſervable, that Titus Andronicus, the moſt outrageouſly bloody, is alſo, the moſt flat and inſipid of all the plays of *Shakeſpeare*, if, indeed, that mean performance is from the hand of *Shakeſpeare*. Thoſe, who are converſant with ancient metrical Romances and Ballads, muſt be ſenſible of the truth of this remark; they muſt perceive, that thoſe productions of rude and infant taſte are, in general, deficient in vigorous thought, poetical deſcription, and in all thoſe ſublime conceptions, which characteriſe the true Poet; but, in return, they abound in goblins, giants, witches and enchanters, with a plentiful ſeaſoning of cruelty and blood. Such is the ballad called the Lady *Iſabella*'s Tragedy, for example, which I would recommend to ſome German or Anglo-German playwright of the monſtro-terrific ſchool, as affording an exquiſite ſubject, full of delightful bloody deſcription and horrible incident, and preſenting ſituations of the higheſt ſtage effect.

Then

* See his grand Cyrus, &c. &c.

Then ſtraight his cruel bloody hands,
He on the lady laid;
Who quivering and quaking ſtands,
While thus to her he ſaid;
" Thou art the doe that I muſt dreſs,
" See here—behold my knife;
" For it is pointed, preſently
" To rid thee of thy life."
O then cried out the ſcullion boy,
As loud as loud might be,
" O ſave her life, good maſter cook,
" And make your pies of me!"

What follows may be compared with any thing in *Burger*.

" O then," beſpoke the ſcullion boy,
With a loud voice, ſo high,
" If now you will your daughter ſee—
" My lord—cut up that pye,
" Wherein her fleſh is minced ſmall
" And parched with the fire,
" All cauſed by her ſtepmother,
" Who did her death deſire.—
Then all in black this lord did mourn,
And for his daughter's ſake,
He judged her cruel ſtepmother
To be burned at a ſtake.
Likewiſe he judged the maſter cook
In boiling lead to ſtand,
And made the ſimple ſcullion boy
The heir of all his land.

It is on ſuch claſſical models as theſe that one of the moſt favourite and admired *German* writers has formed his ſtyle. Indeed, he has

has not reſted there; he has borrowed from obſcure *Engliſh* ballads the ſubjects of ſome of his poems, and appears even to have imitated particular paſſages.* Yet, though we may trace in *Burger* perpetual marks of imitation, he has contrived, by the uſe of extravagant

* The ſtory of *Leonore* ſeems to have been ſuggeſted by a ballad entitled the "*Suffolk Miracle*," or a relation of a young man, who a month after his death appeared to his ſweet heart, and carried her on horſeback behind him for forty miles in two hours, and was never after ſeen but in his grave. It is in a collection of ballads in three volumes, printed in 1723. The following lines are remarkable:

When ſhe was got her love behind,
They paſt, as ſwift as any wind,
That in two hours or little more
He brought her to her father's door.
But as they did this great haſte make,
He did complain his head did ache,
Her handkerchief ſhe then took out
And tied the ſame his head about,
And unto him ſhe thus did ſay,
Thou art as cold as any clay,
When we go home a fire we'll have,
But little dreamed he went to grave.

The reader will obſerve in the ſame poem lines that are a manifeſt imitation from the antient ballad of William's ghoſt.

The lines in Leonore begin,

And where is then thy houſe and home, &c.

The lines in the Engliſh original begin,

Now has ſhe kilted her robes of green
A piece below the knee, &c. &c.

travagant diction, by an attempt, in many inſtances unfortunate, at Onomatopæia,* and a choice of horrid and fantaſtic ſubjects, to eſtabliſh a character for originality.

It may be remarked, as another peculiarity of the *German* writers, that amidſt their love of horrors and their affectation of the ſublime, they have had the ſingular felicity of finding ſources of the great, the terrible and the pathetic in all that is commonly held to be little, contemptible and ridiculous; and, deſcending a ſtep below domeſtic tragedy, they have introduced a new kind of drama, which, for want of a more appropriate term, may be called ſtraw tragedy, and which climbs into the garret, or dives into the cellar, for its heroes and heroines, and is founded on the loves and heroic acts of beggars and bunters, of thieves and cut-purſes, of tailors and ſeamſtreſſes; on ſuch tranſactions as an inſurrection of journeymen againſt their employers, which has furniſhed Mr. *Foote* with his tragedy for warm weather. The incidents, which to an Engliſh writer appear highly ludicrous, and become the ground work of a *Beggars Opera*, are adopted by a *German* and become the ſubject of an horrible tragedy, full of portentous incident and deep diſtreſs.—*Macheath* and his gang ſoar into the clouds of bombaſt; they moralize on the inequality of human conditions, and conſider themſelves as the vicegerents of providence, commiſſioned to rectify the caprices of fortune. *Polly Peachum*

* By the introduction of cling clang, ſpliſh ſplaſh, ſpatter clatter, rattle twattle, huddle cuddle, hurra hurra.

Peachum and *Lucy Lockitt* perplexed in the extreme, between diſintereſted love and tender friendſhip, reſolve, at the ſame moment, on the moſt heroic ſacrifices, and mutually determine, each to devote herſelf for the happineſs of her lover and her rival. The combat of friendſhip and generoſity is carried on, through many high-wrought ſcenes; at laſt, the young ladies agree to end the ſentimental conteſt, by poiſoning themſelves and their lover, and all three expire together embracing and embraced. *Lockitt* informs againſt old *Peachum*, who is broken on the wheel, for the amuſement of the audience. *Macheath*'s band ſet fire to the priſon, and ſo the piece concludes.

Let not this be thought exaggeration.—Take one of the moſt applauded *German* Dramas, the *Robbers of Schiller*.—What is it but the *Beggar's Opera* tragedized and amplified, with a little ſprinkling of imitation from *Fielding* and *Shakeſpeare?* The hero, after a youth ſpent in profligacy, riot and extravagance, with a ſet of abandoned aſſociates, is expelled from the preſence of his father, who however ſtill loves him, and continued in exile, by the inſidious arts of his younger brother, alike deformed in mind and body, who envies and hates him. *Charles*, thus ſpited at the world, calls together the companions of his paſt vice and folly, forms them into a band of robbers and becomes their captain. They are the terror of the country, and commit every

ſpecies of enormity. *Charles de Moor* all the time, though covered with crimes, impious, ſacrilegious, an incendiary, a murderer, affects to moraliſe on the inequality of human conditions; repreſents this inequality, as a ſufficient plea for the outrages of the robber and the pirate; ſets down his crimes to the account of overruling Providence; and exalts himſelf and his fellow robbers into the ſacred characters of miniſters of eternal juſtice, the predeſtined ſcourges of guilty men, choſen inſtruments, authoriſed and appointed by Heaven, to rectify the defects of ſocial and civil inſtitutions. *Charles de Moor* loves to diſtraction his own couſin, who had been bred up with him from her infancy, and ſhe returns his paſſion with equal ardour. He viſits this lady in diſguiſe; converſes with her, makes love to her, yet, ſtrange to tell, ſhe does not diſcover who he is; and what is yet more ſtrange, and certainly an extraordinary and original ſtroke of dramatic contrivance, though the lady does not recognize her lover, ſhe perceives a very ſtriking reſemblance between the gentleman and himſelf, but ſtops ſhort in this critical point, and actually falls in love with the ſuppoſed ſtranger for his likeneſs to the deceaſed lover, to whom ſhe becomes falſe through an exceſs of love * *De Moor* having thus ſucceeded, in ſeducing his own miſtreſs to exchange the ſubſtance for the ſhadow, and in rivalling himſelf, very abſurdly and unreaſonably, in my opinion, flies from her in a rage, at the moſt tender moment imaginable; and becomes weary of life,

* This is a dramatic ſituation worthy of Bayes himſelf, there is nothing like it in the amours of Prince Prettyman.

life, becauſe his miſtreſs was ready to make him happy—and *Amelia* in a dialogue of high-wrought tenderneſs, which is worthy of being read, as a maſterpiece of abſurdity, having conſented to ſurrender to the captivating unknown,* together with her virtue, a ring, the keep-ſake of her *loſt lover*, *Charles* flies in a fury; but ſoon returns with his band of robbers. He releaſes his old father from a diſmal dungeon, where he is confined and ſtarved, by order of his ſecond ſon. *Charles* cauſes this unnatural relative, who is a ſort of mongrel being, patched up from *Fielding's Blifil*, *Shakeſpeare's Iago*, and *Richard the third*, to be put to death,—kills with his own hand the beautiful *Amelia*, whom he adores, moraliſes and preaches to the robbers, in an exalted ſtrain, and then ſurrenders himſelf to the thief-takers of the Rotation office, that a peaſant, whom he finds by chance labouring on the road, may receive the reward offered by government for the apprehending of him!

Antient critics taught, and the doctrine ſeemed to be founded in truth and nature, that it was the great art and ſkill of the dramatic writer, and the great excellency of dramatic compoſition, to aſſign to each perſonage the language, ſentiments, and conduct ſuited to his country, his age, his ſituation, his profeſſion, and rank in life.

 Deſcriptas

* This is certainly *dignus vindice nodus*.—Both the lady and the audience were in rather an aukward predicament; but their bluſhes are ſpared, and indecorum prevented; for when *Charles De Moor* diſcovers himſelf, and breaks away in a paſſion, *Amelia* ſwoons and the ſcene cloſes. See Robbers.

Deſcriptas ſervare vices operumque colores, &c. &c.
Si dicentis erunt fortunis abſona dicta
Romani tollent equites, peditesque cachinnum.
Intererit multum Davuſne loquatur an Heros,
Maturuſne ſenex an adhuc florente juventa
Fervidus: an matrona potens, an ſedula nutrix,
Colchus, an Aſſyrius, Thebis nutritus, an Argis, &c. &c.
Ætatis cujuſque notandi ſunt tibi mores, &c. &c.

It is the boaſt, of the *German* authors, and their copyiſts, to write in direct oppoſition to theſe good rules, and to the common ſenſe and general experience of mankind, on which they are founded; and to ſurpriſe you perpetually, with ſuch language, ſentiments, and conduct, as you could leaſt expect, ſuch, in fact, as are the moſt foreign and abhorrent from the received notions reſpecting the perſonages introduced. Thus, the ſtage is filled with patriotic highwaymen, chaſte curtezans, liberal jews, ſentimental bailiffs, benevolent executioners, heroic tailors, poiſoning lovers, proſtitute princeſſes, miniſters of ſtate who are buffoons, and veteran generals, who are petit maitrés.* There is however a method in this madneſs. The great object is to aſtoniſh and dazzle, an effect at which the *German* writers and their ſchool are for ever aiming, and to which they readily ſacrifice the maxims of propriety

* The pernicious conſequences of this incongruity in unſettling all our notions of decorum are obvious.

priety and the rules of good writing—Novelty—Novelty—Surpriſe—Surpriſe—are their darling *Cleopatras*; the idols of a falſe ambition, to which we may trace moſt of their peculiarities and abſurdities.

This ambitious ſpirit of the *German* writers occaſions, in all their productions, another *trait* of family reſemblance, which is *exceſs*; a ſtraining at ſomething ſuperlative, an attempt to ſurpaſs nature, that produces only contorſion and grimace. Their incidents are in exceſs, of horror, and of burleſque, that exhibit revolting ſpectacles or contemptible farces. Their perſonages are alſo in *exceſs*; there is nothing of the juſt ſize or proportion of nature, but all are giants or dwarfs. There is no true delineation of character. All the lines are aggravated, all the features are over-charged into caricature. Their heroes and heroines are bedlamites; their comic characters merry Andrews and cinder wenches. When they would depict paſſion, *exceſs*, *exceſs* ſtill predominates. They want the keeping, the reſerve, the chaſtity of manner inſeparable from probability and nature. Their virtues attempt to riſe to ſomething ſuper-human, and fall into their contraries; they are loſt in the clouds of romance and extravagance, or involved in the mazes of chimerical and unintelligible refinement. Their dramatic exhibitions of vice are monſters redeemed by no virtue; they carry their malignity and guilt to an exceſs unexampled

ampled in the hiſtory of our ſpecies, and only to be found in what fancy may have feigned of the diabolical nature.

> Abominable, unutterable, and worſe
> Than fable ever feigned or fear conceived.
> Gorgons, and hydras, and chimeras dire!

The unwearied predilection for a diſplay of the moſt atrocious crimes is peculiarly characteriſtic of the German muſe, and is naturally connected with what we have already noticed, a fondneſs for the exhibition of dreadful ſpectacles,

* Verbera, Carnifices, Robur, Pix, Lamina, Tædæ.

The want of moderation is particularly obſervable in the affectation of exceſſive feeling, of tenderneſs and ſenſibility; in the extreme of naiveté and ſimplicity, without bounds. Innumerable inſtances of this occur in moſt of the German compoſitions with which we are acquainted. This ſentimental ſtyle, this rage of being very very natural, even to a degree of artificialneſs that is diſguſting, predominates too much in the beſt *German* writers, ſuch as *Gæthe* and *Wieland*, and abounds in almoſt every page of *Kotzebue*. The *German* writers, not ſatisfied with this diſplay of exceſſive tenderneſs and ſenſibility, ſeem to conſider the repreſentation of paſſion as the great buſineſs of a poet, and the impaſſionated

* Theſe were all employed in the infliction of torture.

ſionated vehement ſtyle as his great perfection; and in the attempt to pourtray paſſion and feeling there is no diſcretion; theſe writers are all for a raging vein, a *part to tear a cat in.* They tear the paſſion to tatters, to very rags. It is not merely, that ſorrow is ſunk, in ſuch a ſuperabundance and complication of calamity, ſuch an exceſs of torment as real life never experienced, and human nature could not endure; that love is ſublimed into frantic ravings and fiend-like jealouſy, and breaths nothing but poiſon, poniards and ſelf-deſtruction; that in the magnanimous, the heroic, the fierce, the vindictive, all is effort, and you perpetually ſee the writer ſtanding on tip-toe, ſtrutting and ſtraining to reach ſomething extraordinary; but the Germans, in their purſuit of the ſentimental and impaſſioned, write, as if they ſought to perſuade themſelves and their readers, that the indulgence of paſſion is the great buſineſs of life and the great privilege of humanity.

The ambitious deſire of ſhining and producing effect, occaſions no ſmall degree of peculiarity in the language and phraſeology of the *German* School. In productions, which are ſerious, and meant to be elevated and pathetic, the ſtile is too commonly bombaſtic and inflated, and the writer appears to be gaſping after gigantic vaſt ſentiments, or labouring, to remove natural and obvious ſentiments from common apprehenſion, by involved and myſterious expreſſions, and unuſual forms of diction. We diſcern an unwearied

wearied but wearisome affectation of saying even the most common things in an uncommon manner. The object of all this, is to surprise the reader, and impress him with an high opinion of the Author's genius and originality; it may, sometimes, have that effect, but is more certain, of rendering the Writer ridiculous, or unintelligible; it frequently turns a whole composition into a collection of enigmas and conundrums, and promotes the adoption of barbarous phraseology, clumsy circumlocution, and unmeaning verbiage.

Connected with this, another peculiarity may be found in the *German* Writers, both of Tragedy and Comedy; I mean the frequency of appeals to the Deity, the profane application of religious sentiments, and the irreverent introduction of the name of God, of religion and religious terms, together with the profuse admission of oaths and imprecations, which are indiscriminately put into the mouths, of the hardened and ferocious free-booter, and of the delicate and accomplished maiden. All this, no doubt, is intended to produce effect, and energy, strength and sublimity, but must offend and revolt any person, who has a respectful sense of religion and decorum.

I have remarked that the *German* School is fertile in extraordinary license of language, while each writer endeavours to invent new phrases, or coin new words to express his new ideas, or more frequently

frequently to deceive the reader, and prevent his ſeeing, that the ideas thus cloaked are trite and hackneyed. Among theſe modern improvements is the introduction of another peculiarity of the Germans, an unconnected and disjointed ſtyle, full of breaks, interjections and apoſtrophes. The *German* language was improving rapidly under the culture of *Geſner*, *Wieland* and *Leſſing*, and would have received the poliſh and perfection requiſite to make it claſſical, had ſucceeding writers trod in their foot-ſteps; but the temperate and judicious manner, the chaſte ſimplicity, and ſober graces introduced by them, and formed on a ſtudy of the antique, did not ſatisfy the aſpiring writers of the new School; theſe geniuſes, in the rage of ſingularity, and paſſion for effect, adopted this *ſingultive* elocution, (if I may be allowed to coin a term) this mental hiccup, ſpeaking by fits and ſtarts broken ſentences; not content with the mutilation of words, they contract whole ſentences in the ſame manner as we would abbreviate words; they aboliſh all conjunctions and connectives, and in many of their works the ſentences are all ſeparate, like the different effata of an oracle; nor are any ſtops or diviſions admitted, except full ſtops, notes of admiration, or marks of interrogation, . . .—!!!—???
This peculiarity will be better underſtood when I come to illuſtrate theſe obſervations by ſome examples from the *German* writers.

The immoderate length of the *German* plays is one of the firſt circumſtances, which will ſtrike a reader in the peruſal, and may,

perhaps, be conſidered as one of their peculiarities. In this they even ſeem to ſurpaſs the earlier productions of the *Engliſh Drama.* There is not a play of *Schiller's* which would not, if it were to be acted as it is printed, take up at leaſt five or ſix hours in the repreſentation, without allowing a reaſonable breathing-time between the acts. This intolerable prolixity ariſes from want of care in the writers, to mature and digeſt their plans, and reviſe and correct their productions. Thus, the fable is embarraſſed with a number of unneceſſary and epiſodical incidents, by which the attention is diſtracted, and the intereſt is weakened and diluted by the introduction of a rabble of unmeaning, inſipid, and idle perſonages; while the dialogue is overloaded, and the progreſs of the action ſuſpended, by inſipid love eclogues, incoherent rhapſodies of unnatural far-fetched ſentiment, or vapid diſſertations, in the ſtyle of the new philoſophy.

Another peculiarity is a device, which, in a great meaſure, has originated with the ſtage of *Germany*, and from it been adopted by their Engliſh imitators; I mean the immoderate uſe of *Italics* and marginal directions; in fact, the writer of a modern Drama, along with the dialogue and words of his play, uſually compoſes and commits to print a regular Pantomime to match it. This poſition may require ſome explanation. The *German* School, ambitious of obtaining eclat, and profeſſing particularly, to diſplay feeling, and exhibit paſſion, yet, unable, or unwilling, to make the exertions in compoſition

compoſition, which are requiſite to attain theſe ends, have adopted this contrivance, in order, to put the change on the courteous reader, or to call out the exertions of the performer, to ſupply their deficiencies, and imagine or create what they cannot really find. Thus, inſtead of deviſing an intereſting ſeries and contexture of incidents, or endeavouring to put into the mouths of their perſonages natural and pathetic ſentiments, they turn all ſentiment and expreſſion of paſſion into mere deſcription of the looks and geſtures, which the performers are to be ſuppoſed to aſſume, and convert the whole pathos and energy of the Drama into marginal directions, explanatory of what the writer meant to ſay and impreſs if he had been able. Thus the Author flatters himſelf he is writing a play, while, in reality, as I have ſaid, he is only inventing a pantomime, in the place of moving ſcenes and burſts of genuine paſſion. A Writer of this ſtamp tells the actors and actreſſes, in Italics, how they are to look, to move, and even to dreſs themſelves.* It is not ſurpriſing that an expedient, which, by transferring the power and exertions of imagination to the reader, is ſo very flattering to the idleneſs or incapacity of authors, ſhould be generally adopted. Accordingly, we find it in ſuch univerſal uſe, that one half, at leaſt, of every new play is taken up

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* In the table of perſons prefixed to the Fieſco of Schiller the character of each is announced and his perſon and dreſs ſet down.—One is old and formal, another young and handſome, a third gruff and ill featured. One is dreſſed in black, another in ſcarlet, nothing is left to the diſcretion of the actors, the manager, or the property man.

with theſe interpolated directions, theſe confidential hints, from the Author to the reader or the actor. In this new art of poetry, it is to the margin or the parentheſis that the humour, the pathos, and the animation of the preſent crop of Dramatic Pieces is conſigned, by writers, who far from finding it difficult to *pen a whiſper*, find it eaſy to pen a laugh, a grin, a ſmile, a ſimper, a frown, a ſtart, a ſwoon, an hyſteric fit, or a convulſion. In truth, the beſt commentator on a *German* play would be a poſture-maſter or a grimacier.——" Approaching with a malicious air,"—" ſtarting and meaſuring him with a long look,"—" making geſtures on his ſeat like a man who is conceiving ſome great project,"—' *Spiegel* grins a malicious ſmile,"—" laughing with ſelf-conceit,"—" biting his nails with paſſion,"—" ſcratching his head like one in great perplexity,"—" putting the fore-finger of his right-hand on his left-noſtril,"—" ruſhes out in deſpair,"—" turning from her as in a frenzy,"—" follows him to the end of the ſtage, and then breaks out into an infernal laugh,"—" with malignant irony."——Stingleſs, indeed, muſt be this ſame *malignant irony*, if it required a marginal direction to apprize the reader of its quality and intention. Unhappy is that painter, who finds himſelf obliged to write under his performances—THIS IS A BEAR; OR, THIS IS A PORCUPINE. One might, perhaps, trace this fondneſs for graphic illuſtration to the writings of *Sterne*. He is certainly a favourite with the Germans, who in their plays and novels have formed their ſtyle very much, on his abrupt, ſentimental, eccentric manner;

ner; and among other peculiarities of Sterne we find he delights much in a peculiar and minute ſpecification of looks and attitudes.

The writers of the *German* School ſeem to be further diſtinguiſhed by an impoſing affectation of originality. This character they endeavour to ſupport, firſt, by ſaying common things in an uncommon manner, and adopting ſingularities of ſtyle, ſome of which I have noticed; they labour, in the ſecond place, to exalt themſelves as the firſt diſcoverers of bold and hardy truths, which the reſt of mankind were too ſhort-ſighted to diſcover, or too puſillanimous to maintain; and to juſtify their claims to the heroic office of emancipating the human mind, they deal in flighty ſentiments and flimſy paradoxes; they vend, by retail, the crude innovations of the new philoſophy, the pernicious reveries of the anarchiſts. Yet, with all the pretenſions to originality, there are few writers who avail themſelves more of imitation. From the *Engliſh*, in particular, they borrow with an unſparing hand; with a predilection, however, for ſuch as have ſomething excentric in their genius and character, ſomething wild and irregular in their ſtyle and manner*. To fortify, however, their title to originality, they ſhew a generous diſdain of the received rules and maxims of criticiſm, a noble diſregard of correctneſs and impatience of the *limæ labor & mora*.

The

* The Germans ſeem to have a peculiar fondneſs for the antient Engliſh ballads and metrical romances and the modern imitations of them; for the writings of *Sterne*, for *Oſſian*, and above all for *Shakeſpeare*.

The haſte of compoſition, and reluctance to blot out any thing that has once eſcaped from the pen, ſo conſpicuous in the *German* writings, are coupled with a certain oſtentation, of ſtrength and force, ſometimes real, more frequently illuſory, ſubſtituting an incondite harſhneſs, and roughneſs, and an inſane extravagance of ſentiment, character and incident, for nervous manner and vigorous conception. Thus certain bravoes and ſwaggerers think they may aſſume to themſelves the attributes of courage and ſpirit, by diſplaying ferocious manners, an offenſive conduct, and a general contempt of propriety and politeneſs. Here and there you meet vigorous and ſhining paſſages, *unus & alter aſſuitur pannus purpureus*—but they appear like fertile and cultivated ſpots, in the dreary vaſt *Serbonian bog*, of contemptible or diſguſting traſh, where *patience whole is ſunk*; as the reader who tries to wade through the *Don Carlos* of *Schiller* will find to his ſorrow.

Certain it is, that whatever may be the cauſe, we find in the *German* compoſitions a marked neglect of all the commonly received maxims of critical taſte, and of all the eſtabliſhed rules of literary compoſition; a wilful or even ſtudied departure from ſymmetry, conſiſtency and regularity; inſomuch, that ſhould we take up the Art of Poetry of *Horace*, which ſpeaks ſo ſtrongly the language of good taſte and ſound common-ſenſe, and read it along with a courſe of German plays, we ſhould find every precept of the judicious *Roman* inverted by the hardy but ſavage Poets of the north.

In

In the *German* plays there is an open and outrageous violation of the dramatic unities of time and place; the action flies, from country to country, and that often in the courſe of the ſame act, and it frequently embraces a period of many years. But this licenſe is pardonable, or even juſtifiable, compared with the groſs diſregard of the more important unity of the fable. Without requiring the ſevere ſimplicity of the *Greek* Tragedy, and rigidly excluding epiſode and under-plot, common-ſenſe demands at leaſt an integrity of plan, a ſymmetry of ſtructure, a connexion of the parts, with each other, and with the whole, and a conſtant but imperceptible endeavour, in the perſonages and incidents, as they are gradually brought forward, to advance and produce the cataſtrophe. Far from this, a *German* play often combines two or three iſolated principal ſtories with a number of unconnected epiſodes, and introduces a variety of perſonages who do not contribute to forward the cataſtrophe. It is rather a wild Romance in dialogue, than a legitimate Drama. To connect the different ſcenes with each other; to develope in them the different perſons of the Drama, and to explain their ſeveral intereſts and motives; to prepare for, and to mark the exits and entrances of the characters, and to take care that the ſtage ſhould not be left vacant; theſe are matters of which the *German* play-wrights are ignorant, or, if they know them, they are unwilling to ſubmit to the ſtudy and labour which they require. The perſonages are not left to announce themſelves; a *German* play takes care to characteriſe them largely in the

Dramatis

Dramatis Perſonæ! little ſolicitous whether, in the progreſs of the piece, they correſpond with this introductory deſcription. What further explanations thereader expects, he muſt ſeek, rather in the Italics, of the marginal directions, than in the courſe of the dialogue. Equally deficient are the *German* plays, in the decorums, proprieties, and decencies of dramatic character, which are ſhewn, by obſerving the circumſtances, of rank, and age, of time, and place, and making the perſonages ſpeak what ſuits their ſituation, or the emergency of the moment. But in nothing is the irregularity and rude taſte of the *German* School more palpable, than in the ſtrange mixture of the bombaſtic and pompous, with the low and ſcurrilous, in ſtyle; and of the ſolemn and horrible, with the ludicrous and farcical, in incident. The author ſeems to march with a ſlow pace on enormous ſtilts; from which, when we leaſt expect it, he ſuddenly deſcends, or rather falls, to ſprawl and grovel in the mire of vulgar buffoonery. I ſhall, in the progreſs of this eſſay, attempt to point out the ſource of this peculiarity, which begins to re-appear in ſome of our modern *Engliſh* plays; mean time, for examples of the practice, I may refer the reader to the low tippling dialogues of the Robbers in *Schiller's* play of that name, and to the ridiculous character of a general officer, in his *Miniſter*.

I BELIEVE I have now adverted to the moſt ſtriking features of peculiarity in the *German* writers. The reader may ſatisfy himſelf, with the juſtneſs of the repreſentation, by conſulting, at random,

random, any of the numerous progeny of that prolific parent the *German* School. They are ſuch marked *manneriſts*, and there is ſuch a ſtrong family reſemblance in theſe productions, that a fair eſtimate of the genius, the ſpirit, and tendency, of the characteriſtic merits, or demerits, of the whole tribe, may be formed, on the conſideration of a few inſtances. Let us now proceed, to conſider the tendency of the German productions, with reſpect to morals.

The Drama is the higheſt ſpecies of poetry, as hiſtorical painting is the higheſt ſpecies, in the pictureſque art. Nothing can be more uſeful, than to repreſent man, in his various characters and ſituations, with truth and juſtice. Without enquiring too minutely, what *Ariſtole* means, by the phraſe of *purging the paſſions*, it is plain, that dramatic poetry, regulated by the rules of a juſt morality, ſupplies important leſſons, by ſhewing the fatal tendency of guilt, and the inconveniences, that reſult from vice and folly. It impreſſes, in the moſt forcible and pleaſing manner, maxims, which may be collected from paſt experience. It leads us, to controul our paſſions, and regulate our feelings; and to acquire an habitual temperance of mind; by ſhewing the acute ſufferings, which attend the unbridled ſway of paſſions and emotions, not excepting thoſe, which, in themſelves, are laudable. It repreſſes pride and inſolence, by diſplaying the uncertainty of human proſperity, and cultivates the kind ſympathies, by reminding us, that ſorrow

is the common lot of man. It is requiſite to the moral direction of the Drama, that it ſhould give examples of crimes puniſhed, of vice and folly ſubjected to juſt contempt, and of men expoſed to miſery, by the blind indulgence of paſſion and feeling in exceſs. Thus, will the dramatic poet officiate, with pure and pious hands, at the ſhrine of virtue.—Could it even be queſtioned, for a moment, whether the Drama is bound to anſwer the purpoſes of a moral deſtination; yet, ſurely, it cannot be denied, that a Theatre, which inverts all the rules of decorum and decency, and outrages all the principles of religion and morality, ought to be reprobated, in every well-regulated ſociety.

Little inſtruction can be conveyed, little ſympathy can be excited, by the generality of pieces, which iſſue from the *German* Theatre. To produce theſe effects, men muſt ſee their reſemblances on the ſtage, ſomething that comes home to their own breaſts. Such men and women as appear in the *German* plays are rarely met with, in the world, and when, here and there, ſuch do make their appearance, the police of the place (if there is any police) takes charge of them on itſelf, and confines them in Bedlam or the Work-houſe. The favourite characters, moſt frequently diſplayed on the German Stage, are frantic lovers, parricides, highwaymen,* miniſters, miſtreſſes, melancholy and raving perſons of all ſorts; the bulk of mankind can derive but little inſtruction, from the exhibition of ſuch characters.—Far from leading

* See the Plays of Schiller.

ing us to controul our paſſions, and moderate our feelings, the *German* Drama is calculated to operate effects directly contrary; and the peruſal of ſuch writings muſt be peculiarly injurious to young perſons of both ſexes. They repreſent the force of paſſion as irreſiſtible, and all oppoſition to its impulſe as fruitleſs and abſurd, indeed, they go farther, they encourage a blind and headlong ſubmiſſion to the unbridled ſway of paſſion. They even juſtify it, as meritorious, as an act of obedience to the ſupreme decrees of Heaven, a conformity with the unchangeable order of nature. Theſe writings ſap and unnerve the ſoundneſs of the intellect. They feed and diffuſe a prevailing malady of the times, which has taken too full poſſeſſion of the female world, and, indeed, of many men, under the name of *ſentiment*; a malady, which deifies a certain unmeaning, undeſcribable quickneſs of feeling; and exalts a morbid and abſurd ſenſibility, into the perfection of human nature.

I DO not yield implicit credit to all the tales, which *Barruel* and *Robiſon* have publiſhed, reſpecting the character and deſigns of the *illuminated:* but I am ready to admit, that a ſpirit of innovation is afloat. The *genius and the mortal inſtruments are now in council*— and ſome of the moſt ingenious and learned men, not only throughout *Germany*, but in moſt other parts of *Europe*, are unfriendly to the preſent eſtabliſhments in Church and State. The productions of the *German* Muſes, it muſt be acknowledged, bear

 ſtrong

ſtrong internal marks of the affection, which the writers bear to the new philoſophy, and the revolutionary ſpirit; marks, which ſo conſtantly recur, that they cannot be wholly aſcribed to accident. Every topic, which tends to make men diſſatisfied with the exiſting order of things, the reſtraints of law, the coercion of civil governments, the diſtinction of ranks in ſociety, the unequal diſtribution of property, and with the diſpenſations of Providence itſelf. is urged in theſe productions.* Every motive is inculcated, which may induce men to be diſcontented, with the government under which they live, or, indeed, with any government, and to become active partizans of anarchy and diſorder. The miſerable condition of the many, the luxury, the pride, and avarice of the few, the inſolence of office, the corruption of courts, the crimes and vices of kings and ſovereigns, the wickedneſs and oppreſſive arts of their miniſters, are themes of conſtant declamation, and are painted in the blackeſt ſhades of exaggeration. Many of theſe things *we may potently believe, but we hold it not honeſty to have them ſet down.* The precepts of morality, the rules of decorum, eſtabliſhed cuſtoms, received opinions, and even the principles and ſanctions of religion are treated with contempt, and exploded as vulgar errors, and prieſtcraft, fitted only to impoſe on weak underſtandings, and overawe little minds. The doctrine of abſolute decrees and fataliſm, the irreſiſtible power and unqueſtionable dominion of paſſions are inculcated; and their influence and ſway held

* See the Robbers and *Miniſter* of Schiller paſſim.

held up to view, as a full excuſe for the broadeſt deviations from rectitude, the groſſeſt enormities of conduct. The heroes are robbers, cut-throats, ſuicides, poiſoners and parricides. The heroines are devoid of chaſtity, the ſlaves of paſſion, fearleſs of ſhame, unawed by God, they talk blaſphemy and call it ſentiment. The blaſphemous exclamations againſt Providence, in the *Leonora* of *Burger*, the ferocious and criminal rhapſodies of *Charles De Moor* and his aſſociates, in the Robbers, and particularly the *Miniſter* throughout, may ſerve to eſtabliſh and illuſtrate my aſſertions. In fact, the writers of the *German* School ſeem to imagine, that, as the imitative arts have the phyſical power of repreſenting objects good and bad, pleaſing and hideous, ſo, their profeſſors have the dangerous privilege of exhibiting to public view every object, that lies within the compaſs of phyſically poſſible repreſentation, without regard to the principles of ſound morality, or the rules of correct taſte, which forbid the repreſentation of ſome things, as licentious, and criminal; of others, as too horrid and diſguſting.

It is well obſerved, in the Monthy* Review, that the claſs of ſtories, ſuch as are the favourites with the *German* writers, "by "familiarizing characters of a ſtronger ſinew than are common, "crimes of a bolder enormity, and modes of coercion, which the "tolerance of a poliſhed age had renounced, tend to ſuggeſt a re-"vival of the heroic in virtue and vice; and to prepare the mind, "for contemplating with complacency a ſort of characters, the in-

fluence

* See the Article *Herman of Unna*. Vol. 15, New Series, page 21.

" fluence of which may not prove very compatible with the mo-
" notonous tranquillity of modern ſtates."—Nor are the *German* productions contented with the diſpoſing us to view ſuch characters with complacency; they lead us to imitate them; and they tend to rouſe and ſtimulate ſuch characters to action, and prepare them for ſome theatre, where they may diſplay their dangerous energies. Theſe effects are produced, by venting, as I have ſaid, the poiſon of anarchy, under the ſemblance of bold and hardy truths, reſpecting government and religion; and by introducing topics of declamation, calculated to render men diſſatisfied with their preſent ſtate, diſaffected towards the conſtitution, and order of things, under which they live, and even hoſtile to all civil inſtitutions. Social ordinances and human reſtraints of action are decried, as means of depreſſing the man of feeling and generoſity, the genius, and the hero, for the benefit of the fool, the miſer, the coward and the ſlave. Civil polity and forms of government are the themes of ſatire; and their defects and inconveniences are oſtentatiouſly diſplayed and ſtudiouſly exaggerated; ſovereigns and their miniſters are depicted, as monſters of vice and inhumanity, the natural enemies and ſcourges of their fellow creatures. While the mind of the reader is ſoured and blackened, by theſe gloomy and diſguſting pictures; he is inſtigated to the wildeſt exceſs of paſſion, by declamations, that decorate, with the luxuriant ornaments of fancy, every ſpecies of criminality, every form of madneſs, and folly; and repreſent ferocious jealouſy, mortal hatred, ſanguinary vengeance, ſuicide, and murder, as the marks, or the effects of a manly and energetic

gelic character. Even the Deity himſelf is impiouſly pourtrayed, as the inſtigator and accomplice of human crimes.

The tenet of blind *fataliſm* and abſolute decrees, ſo induſtriouſly diſſeminated* by the *German* writings, is a doctrine, which goes directly to make men forget, that they are rational and accountable creatures; and to ſubvert all the reſtraints of conſcience; and thus to promote a certain ferocity of character, an unrelenting hardneſs of heart, and an abandonment of the will to all the incitements of domineering paſſion, all the ſuggeſtions of diſordered imagination. We have ſeen, both in *Mahometan* and Chriſtian fanatics, the dreadful effects of this implicit belief in predeſtination. We may well conceive the pernicious effect of thoſe productions, which, while they tend to keep the mind in a ſtate of effervescence, and wind up the paſſions to fury, endeavour to perſuade us, that any reſiſtance to their frenzy is an oppoſition to the decrees of God; and, of courſe, that a blind indulgence of them is not only juſtifiable, but meritorious; and thus combine the two moſt dreadful and ungovernable ſprings of human action——*enthuſiaſm* and *fataliſm*.

In the *German* Plays, the moſt abominable characters, wretches borne away by every irregular paſſion, and plunged in every criminal exceſs, are introduced, as principal perſonages, hold the chief poſſeſſion of the ſtage, and are made to utter ſentiments ſuitable

* See abundance of declamation to this effect in *Schiller*.

able to them, cloathed in correſpondent language and expreſſions.* Now, it is ſaid by *Ariſtotle*, that the manners in Tragedy ſhould be good——good—in the uſual ſenſe of moral goodneſs.—" The " reaſon of this precept (ſays his ingenious tranſlator *Mr. Twining*) " Ariſtotle has not given, but it appears from his ſubſtituting the " word βλαβερα, hurtful, pernicious, for μοχθηρα πονηρα, in his enu- " meration of the greateſt faults of poetry, at the end of chap. 25th, " that, however he might differ from *Plato*, as to the hurtful ten- " dency of Tragedy, and of imitative poetry, in general, he " agreed with him as to the danger of admitting poetical, embel- " liſhed, and flattering pictures of vice; in which, as one of the " moſt eloquent, and I might add one of the moſt platonic of " modern writers, expreſſes it——L'Auteur, pour faire parler " chacun, ſelon ſon caractere, eſt forcè de mettre dans la bouche " *des Mechans* leurs maximes & leurs principes revetus de tout " l'eclat des beaux vers & debitès d'un ton impoſant & ſenten- " tieux pour l'inſtruction du parterre.—With reſpect to characters " of atrocious villainy, ſuch as that of *Glenalvon* in *Douglas*, which " can only excite pure deteſtation, the ideas of *Plato*, and per- " haps of *Ariſtotle*, were nearly the ſame, as thoſe which this ad- " mirable writer has expreſt, in the concluding note of his " *Nouvelle Heloiſe*. En achevant de relire ce recueil, je crois " voir pour que l'interet, tout foible qu'il eſt, m'en eſt ſi agreable " & le ſera je penſe a tout lecteur d'un bon naturel, c'eſt qu'au " moins

* See Charles de Moor in the Robbers, Fieſco, in the play that bears his name, the miſtreſs and the lover, in the *Miniſter*.

" moins ce foible interet eſt pur & ſans melange de peine; qu'il " n'eſt point excité par des noirceurs, par des crimes, ni melè de " tourment de hair. Je ne ſcaurois concevoir que plaiſir on peut " prendre a imaginer & compoſer le perſonage d'un ſcelerat, a ſe " mettre a ſa place tandis qu'on le repreſente, a lui preter l'eclat " le plus impoſant. Je plains beaucoup les auteurs de tant de " Tragedies pleines d'horreurs, les quels paſſent leur viè, a faire " agir et parler des gens, qu'on ne peut ecouter ni voir ſans ſouffrir." —How different are the notions of the German Dramatiſts! They ſcarce ever think of exciting intereſt, but through the medium of crimes and horrors; to communicate the torment of hating is their favourite object, and to imagine and compoſe the perſonage of ſome monſter of guilt and depravity, is their delight. With what ſenſations, would Rouſſeau have peruſed the plays, of Goethè, or Schiller!

When robbery, murder, ſuicide, and every other form of atrocious guilt is clothed by the poet in pompous language, decked with impoſing colours, and perſonified in his heroes and heroines, or aſcribed to thoſe characters, which he labours to exhibit as amiable or eſtimable; and when theſe enormities are juſtified, by plauſible pretences, and grave arguments, or even panegyriſed, as acts of virtue and heroiſm, in ſwelling declamation, what muſt be the effect on the minds of the young and unexperienced! How muſt ſuch compoſitions tend, to pollute the minds, and deprave the morality of the riſing generation! Look into the *Robbers* and

the *Miniſter* of *Schiller*, and the play of *Kotzebue* beſt known by the name of *Lovers Vows*, and you will find this exemplified in a manner, that implies a ſyſtematic and rancorous hoſtility to virtue, ſobriety, decency and good order. Indeed, ſome parts of theſe admired Dramas may be ranked with the moſt vicious effuſions of the preſs. In the Robbers, the author, that he may make the murderous crew, the aſſociates of his hero, talk in character, fills the dialogue with horrid oaths and imprecations, with blaſphemy and ribaldry, worthy of the refuſe of a guard-houſe, or a gaol. Nor do the German plays confine the uſe of oaths and imprecations, the diſplay of profane and impious ſentiments, to characters which are meant and profeſſed to be drawn as ferocious and cenſurable, to robbers and aſſaſſins. We find them aſcribed to females, nay, to females which the poet announces as feminine, good, and amiable, and exhibits, as objects of imitation, to their ſex. This hurts probability, as a violation, of dramatic decorum, and conſiſtency of character, and of that adherence to the appropriate manners, which the Drama requires. But every man, who has a ſenſe of religion, or a regard to decency and good morals, will find much more weighty objections, to a practice which ſports irreverently with the name of God, and leads to irreligion and profanation; and which muſt be particularly injurious, as holding out to the female world, an example, which I fear is ſuperfluous to many among them, of manniſh manners, and bold ferocity.

But amidſt all the liberty, which the German writers arrogate to themſelves, of outſtepping the pale of modeſty, and decorum, and

and uttering bold and licentious truths; with all their pretenſions to enlighten a beſotted world, to clear away vulgar errors, and diſpel the miſts of prejudice; they fail to communicate, that moſt uſeful acquirement, a knowledge of men and manners.—Do you ſeek in their Dramas to find inſtructive pictures of life—to *catch the manners living as they riſe*—to trace the plaits and foldings of the heart, and mark the various ſhades and colourings of human weakneſs and frailty?—Merciful Heavens! how will you find yourſelf diſappointed!—I might have obſerved, in my review of the peculiarities of the *German* School, the total abſence of wit and humour from its productions (and they are very numerous) which profeſs to be comic. Of witty writings, if the *German* language affords any ſpecimen, I know of none that has ſtood the teſt of tranſlation.* In their various attempts at humour, the *German* writers have been peculiarly unfortunate. In the place of humorous character, we find unnatural hard caricatures; in the place of humorous dialogue, coarſe and vulgar ribaldry, and in the place of humorous incident, extravagant buffoonery, and the meaneſt puerilities, and loweſt form of farce.—Suppoſe ſome perſon, who had been kept in utter ſecluſion from the world, and debarred of all views of life, and all means of forming opinions reſpecting men and things—put the German writings into the

 hands

* There is a plentiful lack of wit and humour, in the German Comedies, moſt of which are of the ſentimental claſs, and preſent, here and there, it muſt be confeſt, ſcenes of true pathos. The *German Spectator*—The *German Gil Blas*, *Sebaldus Nothanker*, &c. &c. do not form an exception, if we may judge of them in tranſlation.

hands of this perſon, and tell him, from them, to frame his judgment of mankind, their diſpoſitions, and their dealings—What would he think?—He would conſider his fellow creatures as peſtilent monſters of guilt and folly.—He would thank Heaven, that in his ſequeſtered Lazaretto, he had ſo long eſcaped the plagues of ſociety, and been removed from the contagious commerce with mankind.—"I have ſeen enough, (he would cry) remove me from "this congregation of fiends and ideots; return me to my ſolitude; "cloſe the door for ever; and let me forget, if poſſible, that I am "a man."

HAVING conſidered the peculiaries, not only of ſtyle and manner, but alſo in the choice of ſubject matter, that diſtinguiſh and disfigure the writings of the *German* School; and the obvious tendency, and natural effects of ſuch productions, viewed in a political light, with regard to their influence on moral opinion, on the temper of the human mind, and the conduct of men in ſociety; one is naturally borne by the current of thought, to conſider ſome of the moſt prominent cauſes, which have given birth to the peculiarities in queſtion, and impreſt on the *German* writings their ferocious character, and revolutionary bias.——With reſpect to the general coarſeneſs of ſtyle, and the abſolute want, or the perverſion of taſte, it is to be obſerved, that one of the moſt notorious cauſes, of a perverted taſte, and defective judgment, is ignorance of life and manners. The more people mix with each other

other, the more will their taſtes, as well as their manners be refined. The aſperities of temper and character will be worn down, by colliſion. They will begin to ſee the beauty and value, of moderation, and decorum, to eſtimate and ſtudy the arts of pleaſing. They will begin to ſee, that licenſe is not always true liberty; that reſtraint, and a ſubmiſſion to rule and method are not an abridgement of true happineſs, in real life; or an impediment to true genius, in the productions of literature. The effervescence of ſelf-conceit will be checked; men will become acquainted with the opinions of others, and be impreſt with the neceſſity of paying ſome deference to thoſe opinions. All this will produce an acuteneſs and readineſs of the moral ſenſe, an improvement, and practical direction of the diſtinguiſhing faculty, which will not fail to ſhew themſelves, in a mild and placable demeanor, in a regulated and decorous morality; and in a ſtyle of writing pure, correct, and noble.

Literature cannot make a more valuable preſent to the world, than views of life and manners, drawn with a faithful and correct hand, provided, it takes care, to mark vice and folly, with contempt, and the more rank and vigorous enormities, with the deteſtation, which they naturally excite, in the ſound and undepraved boſom; at the ſame time, avoiding ſuch pictures of groſs ſenſuality, as are drawn from the haunts of vice, and are fitted to inflame the paſſions, and pollute the imagination; and ſuch black and exaggerated diſplays of atrocities and crimes, as can only excite pain and diſguſt, and diſpoſe us to miſanthropy, by repreſenting human nature

ture brutalized and diſgraced ; at once, an object of ſcorn, averſion, and terror. Such views of life, at once inſtructive and amiable, are not to be found in the *German** productions. I cannot ſay, that they abound in tender enervating ſentiments, and voluptuous deſcriptions of ſenſual indulgence, ſuch as prevail in the *Mileſian* Fables ; but what is yet more dangerous, particularly to young perſons, of a ſanguine temperament, and ſerious diſpoſition, they are filled with a ſort of amorous myſticiſm, which perverts the devotional language and enthuſiaſm, and applies them to the commerce of the ſexes. They deal out extravagant and romantic flights of ſentiment, a licentious and declamatory parade of ungoverned and ungovernable paſſion ; and utter, with a ſort of oracular confidence, maxims of ſelf-devotion, of frenzy, and exceſs, calculated, to heat the fancy, bewilder the head, and debauch the heart ; to people the ſtews, the priſon, and the mad-houſe, with a numerous and miſerable tenantry.

A WRITER cannot communicate what he does not poſſeſs. There are various cauſes, which render an extenſive knowledge of men and manners leſs acceſſible, to *German* writers, than to others, and render the views of ſociety, and the ſamples of human character and conduct, which they are able to obſerve and collect in the courſe of their experience, leſs proper to be laid before the world, for its inſtruction and imitation.

IN

* I muſt always be underſtood to ſpeak of the more modern German ſchool.—— In the preceding writers of the native language, ſuch as Geſner, Klopſtock, and Wieland, &c. &c. correctneſs, ſublimity, truth, nature, a pure morality, and the moſt amiable and affecting graces abound.——Omnia ſi ſic !

In the *German* towns, the different claſſes of people do not mix ſo much together, as in other countries. This is owing to a certain ſtiff and unſocial pride, religiouſly tenacious of rank and place. The *German* in middle life, (in which ſtation the authors by trade are commonly found) has no acceſs to any thing, which bears the name of nobility, belongs to nobility, or appertains to the court. The taſte for ſocial pleaſures, and the elegant unbendings of poliſhed life, where orders, and degrees, and dignities loſe themſelves, in one maſs of elegance and pleaſures, in a republican fraternity of enjoyment, are more confined in the *German* cities, than in thoſe of moſt other people; conſequently, the inhabitants do not enter ſo freely into the innumerable incidents and varieties of common ſociety, and the boundleſs diſplay of character, which the temporary levellings of civilization and urbanity neceſſarily produce. Such reſtraints on the intercourſe of men, as prevail in *Germany*, for the moſt part, being attended by this want of intereſt, in the uſual virtues, vices, and foibles of men, this inſenſibility to the little events of ordinary life; the writers of this country are driven to look for ſtrange events, outrageous emotions, and monſtrous caricatures, in order to excite attention. Authors, who court the favour of ſuch judges, muſt deprave themſelves, to the ſtandard of thoſe, whom they ſeek to pleaſe. A ſtate of ſociety, where the intercourſe of man with man circulates freely, and converſation is open and uninterrup'ed, offers to the literary adventurer a fairer and more enlightened tribunal. The readers are ſatisfied

tisfied with pieces, where the plots are more finely wrought; they are able to judge of the differences of character, and the truth of repreſentation, and willingly ſee on the ſtage ſuch perſons, as they have been accuſtomed to obſerve in the common intercourſe of ſociety. It is remarked, by a judicious traveller,* that the *Saxon* Dramatic Writings are not ſo monſtrous and extravagant, as thoſe, which are exhibited in the weſtern and ſouthern parts of *Germany*; becauſe, in that country a freer intercourſe of man with man, and a more enlightened morality prevails. The poet is there more competent, to draw pictures of human life; and the people are more competent, to judge of the fidelity of the portrait. In other parts of *Germany* the majority of the people conſiſts of a mob, and a mob delights in ſcurrile mirth, and reſorts with eagerneſs to a funeral or an execution. "The poets (continues this ſame traveller) "are often as ignorant as the mob, of the ſprings that "actuate mankind, and have only choſen writing, as a trade. "Their writings, conſequently, have neither beauty, ſhape, grace, "nor proportion."

THERE is another circumſtance, which may have a conſiderable effect. Courts are the ſeminaries of affected manners, ambitious ornament, and the deſire of diſtinctions. Every thing there is eſtimated by exterior, and ſet out for ſhew; every object, every action,

* Rieſbeck.

action, even the moſt trifling, is marked by the deſire of ſhining, and obtaining precedence, and conſideration, beyond its real value. An artificial and overſtrained politeneſs, finery beyond the circumſtances of the perſon, a ſolicitude to excel in uſeleſs and frivolous accompliſhments, importance attached to trifles, an artful concealment of the real ſentiments, a ſpirit of emulation and envy, a habit of crouching to ſuperiors, and treating inferiors with civil inſolence.—The conſequence of all this muſt be to deteriorate the mind, and to cauſe the prevalence of mean ideas.—When the liberty of *Rome* was extinguiſhed, and eunuchs governed the maſters of the world, the mighty Roman genius was emaſculated—general morality was relaxed, by the example of a corrupt and frivolous court.—Taſte was deſtroyed and eloquence expired, while the public mind was degraded and enfeebled, by the preſſure of a weakly ferocious, and idly ſanguinary government.—The frigid affectation of *Seneca*, the falſe and tinſel glitter, and the weariſome Antitheſis of the younger *Pliny* ſucceeded; until the ſun of Roman literature ſet, in the barbarous jargon of the Auguſtan hiſtorians, and the Mooriſh idioms, and impure fables, of the romancer *Apuleius*.—A *Claudian* appeared, it is true, but this was only a purple gleam in the weſt, a luminous diſplay of tranſient glory, announcing the departure, and cauſing men the more to regret the abſence of the illuſtrious orb of day. If ſuch is the neceſſary influence of a deſpotic and luxurious court on taſte and literature, what muſt be the ſituation of taſte and literature, in *Germany*, which abounds,

nay ſwarms, with courts, deſpotic, luxurious, inſignificant, and corrupt?—The influence operates more generally, in example and oppreſſion, to the depravation of man, by reaſon of the confined ſphere in which it acts. A thouſand pigmy ſovereign potentates, with minds as barbarous as their titles, at once, weak and oppreſſive, contemptible and terrible, have, each his little puny ſtanding army, his court, and his courtiers, his maſter of the revels, his maſter of the hunt, and his prime miniſter, his train of comedians, his train of courtiſans, and his train of flatterers.

The civil and religious tyranny, which prevail through the country, and check the free circulation of opinion, prevent the growth of knowledge, and the expanſion of intellect. Nations, which are held, by civil and religious terrors, in the trammels of ignorance, muſt, of courſe, become ſtupid, and depraved, in their taſtes and judgments; they muſt reſemble children, and will, therefore, be delighted with ſuch things as pleaſe children, with the monſtrous and unnatural, with ſtories of bloody murders, and legends of ghoſts and goblins, of giants and enchanters. It is the natural effect of ſuperſtition, to retain men in a certain groſſneſs of manners. While it prevailed in England, the moſt rude and barbarous ſtage repreſentations (the old myſteries, for inſtance) were received with applauſe. The public taſte, in moſt parts of Germany, (I mean the popular taſte among the maſs of the people) is little more advanced at this day than it was two centuries ago in

in *England*. The exceptions to this general cenſure are found in thoſe provinces, where the reformed religion has prevailed moſt. The infinite number of eccleſiaſtical eſtabliſhments and principalities, which overſpread *Germany*, are ſo many nurſeries of ignorance, ſullenneſs, barbariſm, and depraved taſte, while politeneſs of manners, good taſte, and good writing, flouriſh moſt, in thoſe parts of Germany which have the freeſt government.*

THERE is another cauſe, which may have contributed to the rude uncultivated ſtate of the *German* Muſe, and to the unformed taſte, which pervades her productions, in the vernacular tongue. The men of letters in *Germany* have but very lately applied themſelves to the ſtudy of their native language. They ſeemed to deſpiſe it, and, of courſe, remained ignorant of its capabilities, and force; while they addicted themſelves, with eager enthuſiaſm, to the ſtudy of the *Greek* and *Latin* claſſics, and to that ſpecies of criticiſm, which, under the name of philology, exhauſts the time, the labour, and the talents of learned men in microſcopical reſearch, and beſtows a minute and painful attention on words, ſyllables, and commas. I do not mean to diſcredit the painful toils, of thoſe induſtrious ſcholars, or to deny their utility in the republic of letters. I would only ſuggeſt, in this place, that a general devotion among the *German Literati* to ſtudies of this kind,

 may

* The continual ſucceſſion of civil wars is a further cauſe of the backward ſtate of the Germans.

may have concurred, with other cauſes, to produce an inattention to the native language of the country. We ſee, that it is of late years only, that *Germans* of genius and talent have thought of paying their addreſſes to the graces and *muſæ amæniores* in the vernacular tongue. We cannot, therefore, be ſurpriſed, that the attempts of ſuch new beginners, in the walks of fancy and amuſement, ſhould have many of the marks of rudeneſs and inartificial compoſition, which uſually characteriſe firſt eſſays. And yet it muſt be obſerved, that the firſt productions of the *Germans* in their own language are the beſt. The *Idyls* and Death of *Abel* of *Geſner*, and the *Meſſia* of *Klopſtock* are correct claſſical and beautiful productions. The works of *Wieland* abound in natural and touching beauties, in amiable ſimplicity, and delicacy of ſentiment. They are the hands, the impure hands, of the preſent horde of writers, that have polluted the *German* Muſes, and converted them into camp trulls, and drunken viragos.*

It is not ſurpriſing, that the *German Literati* and *Beaux Eſprits* ſhould have been firſt led, to try their ſtrength in the dead languages, or in the more ſevere purſuits of ſcience. The harſh and untuneable nature of their language, embarraſſed, as it is, with guttural conſonants, ſeemed very unfavourable to the exertions of poetry and eloquence, preſented conſiderable difficulties to the firſt

* That the Germans were not naturally deficient in poetical talent, appears from the large collection of Latin poems, entitled—Deliciæ Poetarum Germanorum.

firſt improver, and made the attempt to poliſh and refine what ſtruck the firſt view, as uncouth and ſavage, appear almoſt viſionary. It ſeemed, like the deformed and impenetrable armour of ſome ſhell-fiſh, to defy all endeavours to reach the ſoft, the delicate, and wholeſome ſubſtance that lay concealed beneath. But when ſome bold adventurers had diſcovered the interior and latent merits of the language, and it appeared from the ſucceſsful experiments of *Geſner*, *Leſſing*, *Klopſtock*,* *Wieland*, and others, that it not only poſſeſſed energy, but was capable of grace and melody —why were their ſteps deſerted?—Why were the German Muſes proſtituted, to the ruffian fury, of caprice and ill taſte?—Why was a new-diſcovered region ſo ſoon overſpread, and deſolated, by a barbarian horde?

The firſt writers of *German* poetry ſeemed like magicians, who had broke a powerful ſpell; and no ſooner was the charm, which impoſed ſilence on the *German* writers of their native language, diſſolved, than their loquacity paſt all bounds, and they ſeemed reſolved, to make themſelves amends, for the taciturnity of times paſt, by effuſions without meaſure, and without end. The haſte of writing, and the rage of publication, aggravated the imperfections inſeparable from the unformed ſtate of the language, and the original ignorance of the writers. Immoderate ambition,

* *Klopſtock* has even attained to an harmonious verſification, particularly in his Lyric Poetry, which fully proves, were there no other inſtance, that the German language is very ſuſceptible of grace and melody.

bition, and thirſt of applauſe, joined with thirſt of gain, to encourage an inordinate fertility, and precipitate on the world a ſwarming hoſt of writings, not leſs numerous than the armies which the peopled north——

> Poured from her frozen loins to paſs
> *Rhene*, or the *Danaw*.

Avarice, avarice, if I miſtake not, has the greateſt ſhare, in recruiting for this amazing literary population. It is obſervable of *German* writers, that they have carried the avarice and venality, as well as vendibility, of literature, and the craft or myſtery of authorſhip, to an higher pitch than any other ſet of people. A Fair, or open market, for the ſale of books, is a phenomenon peculiar to *Germany*; and has, no doubt, contributed largely to diffuſe a venal ſpirit among her writers, and encreaſe the unworthy arts of book-making. This mercenary ſpirit is the diſgrace of talent, and the bane of the true intereſts of literature. It produces a deflexion from the walk, which his genius points out to every man, who conſults it; while writers are more anxious, to produce the fruits, which the preſent call of the market requires, than thoſe, which are the free and natural growth of their talents. It occaſions a diſgraceful catching at unworthy and improper ſubjects, merely becauſe they are the topics of faſhionable tattle, and popular rumour; and it produces a greater ſolicitude, to ſwell the quantity, than to improve the quality of literary productions. The inordinate deſire of gain, thus proſtituting talents to the purpoſes

poſes of temporary ſucceſs, compels a poet to ſubmit his taſte and judgment, to thoſe of the audience, or the readers, by whom his pockets are to be filled; and the majority of theſe are probably the worſt judges among the people. It is through this deſire of gain, and the haſte, and immodeſt incorrectneſs, its natural attendants, that *Kotzebue*, who is certainly a man of feeling and genius,* has overwhelmed the ſtage with abortive heaps of crude imperfect productions, many of them offenſive to decorum and good morals. And ſo prolific is his muſe, that the wide extended range of *Germany* ſeems too narrow, for the triumphs of her fecundity; and ſhe ſeems to aſpire to compleat the conqueſt of haſte and incorrectneſs over the *Engliſh* ſtage.

The force of example is another cauſe, to which we may aſcribe much of the depraved taſte of the *German* writers, and particularly the abſurdity and groſſneſs of their Dramatic Muſe, which may be traced up to an indiſcriminate admiration and implicit purſuit of vicious models. The excentricities and failings of one writer, of ſuperior genius, or diſtinguiſhed reputation, produce a cloud of buzzing inſects, who, unable to reach the ſublimities and excellencies of their original, are very competent to copy his defects, and pride themſelves in the partial and deformed ſimilarity of faults alone, as though it gave them a compleat reſemblance to their great archetype. The peculiarities of *Goethè*, joined to the high

* Some ſcenes in his *Benyowſki*, and *Lovers Vows*, evince it.

high name, and poetic rank, which he has acquired, have done great diſſervice to *German* taſte. *Goethè* may, in particular, be called the dramatic father of *Schiller*, and the great patriarch of the terrific and ferocious ſchool. It may be ſaid, that the cannibaliſm of the theatre, the reign of terror and of blood in the Drama, were eſtabliſhed by the *Goſs with the iron hand* of this writer. The ſubject of this piece is the peaſant's war under the Emperor *Maximilian*, and it is filled with all the atrocities incident to the ſubject. The appearance of *Goſs*, like a magic wand, produced, at once, an hundred geniuſes out of nothing. Blind to the real beauties of their archetype, the imitators of *Goethè* have tried to diſtinguiſh themſelves, by copying his extravagancies and faults; and his ſucceſs has led many to imagine, that nothing more is requiſite to form a man of genius, than the becoming bold, impudent, and careleſs about ſtyle and language, entertaining a contempt for every thing, that is called regularity and order. Study and attention to rules were held to be unneceſſary. It became a principle, among theſe writers, that a true genius requires no education, but has all the powers of creation within himſelf; that real judgment and rational principles only make aſſes of men; that dreams, and enthuſiaſtic raptures in his own greatneſs, and the littleneſs of the world around him, are the perfection of man; that the cares and occupations, by which his daily bread is to be provided, degrade him; and that unreſtrained imagination raiſes him to the rank of a divinity.

These extravagant reveries, ſo unfavourable to regularity and correctneſs, in literary compoſition, and human conduct, were obſerved, with ſecret complacency, by certain men, of philoſophical minds, and profound views, who had long been unfriendly to the ſubſiſting eſtabliſhments in *Germany*, (a ſentiment which the prevailing abuſes rendered, in ſome degree, excuſeable) and who looked, with ſolicitude, for ſome general movement of the people, which might bring them onward, to a nearer proſpect, of reaching their ideas of platonic perfection in government. Theſe men were not ignorant, of the influence of poetry, and the ſtage. They knew alſo that *Germany* ſwarmed with productions of the preſs; and were ſenſible, that, by taking poſſeſſion of the theatre, and the printing-houſe, they gained two moſt powerful auxiliaries, two moſt operative inſtruments, for the advancement of their aims, and the diſſemination of their *propaganda*, through the general mind. They hoped, by encouraging the ſway of feeling and paſſion, and diffuſing wild and paradoxical notions, to ſap the foundations of antient authority, and received opinion, and to inſpire an ambitious love of novelty, a ſceptical impatience, of precedent, and tradition, and an hardy ſpirit of inſubordination, which were calculated to unſettle the minds of men, to lead them to extraordinary enterpriſes, and to fit them for acting a part in revolutionary movements. The ſupineneſs of the different governments, in over-looking the obvious tendency of ſuch productions, is ſurpriſing; and it is yet more ſtrange, that many of

the apoſtles and poets of this new and formidable ſect aroſe in the midſt of courts, and were to be found in the boſoms of deſpotic ſovereigns.

The *Germans* are fond of imitating ſome of the writers of *England*. *Shakeſpeare*, in particular, is an object of peculiar veneration among them. It is probable, that a ſervile, yet diſtorted, imitation, of this immortal bard, in his very defects and rudeneſſes, ſuch an imitation, as the painters call *anamorphoſis*, or diſtorted likeneſs, has produced a neglect of rules, a wildneſs of manner, a croud of uſeleſs perſonages, a multiplicity of unneceſſary incidents, an unclaſſical and revolting mixture of buffoonery and bombaſt; a complication of low and ludicrous incidents, with others of a ſerious, bloody, and terrific ſtamp. At leaſt, the concurring example of ſuch a genius as *Shakeſpeare*, has contributed to inflame the national propenſity to the wild, the monſtrous, and the terrible, and to juſtify the total diſregard of rule and method.

Yet ſtill, perhaps, the cauſes, which I have hitherto enumerated, are inſufficient, to account for the peculiarities of the *German* writers, eſpecially for the ſtrange and ſingular prevalence of the gloomy and horrible through their compoſitions. I apprehend, we muſt ſeek for ſome principle of diſtinctive and ſeparative national taſte, more powerful and more deeply ſeated.—Shall we not find this principle, in a powerful enthuſiaſm, not unmixed with wildneſs

wildneſs and ferocity, which a moderate acquaintance with hiſtory will convince us are ſtrong ingredients, in the *German* national character?—In conſequence of this predominant temper, the civil commotions of the *Germans* have been peculiarly obſtinate and bloody; and we ſhall find, that they have been perpetually agitated, with an eagerneſs nearly bordering on frenzy, in the purſuit of ſome ſtrange object.—All *Germany* was, at one time, aſſailed by the myſterious terrors of the ſecret tribunal.* From this unſeen and awful controul, no height of power was exempt. It could reach the moſt diſtant, with its inviſible hand. It filled the ſtrongeſt holds with diſmay, the courts and palaces with mourning.—The attachment to religion was carried to an exceſs, and produced in *Germany* bloody tumults, cruel ſeditions, wild cruſades, and horrible perſecutions. Religious bigotry was inflamed to the higheſt pitch of ſanguinary fury, by contending ſects. The groſſeſt and moſt deſpicable abſurdities were joined, with deteſtable cruelty, and tremendous rage. An example will readily preſent itſelf, to the recollection of the reader, in the tragedies, acted and ſuffered by the Anabaptiſts, under their leaders *Matthias* and *Bocold*, at *Munſter*.—The *Germans*, living in regions which abound with mines, early applied themſelves, and with conſiderable ſucceſs, to experimental reſearches, and made rapid advances, in the knowledge of chemiſtry. Many of the ſecrets, which they thus

 diſcovered,

* There is an intereſting account of the ſecret tribunal in the work called *Herman of Unna.*

diſcovered, appeared ſurpriſing, not only to the vulgar and illiterate, but even to learned men, at a time when natural philoſophy was in its dawn.——What was the conſequence?—Chemiſtry ſpawned an illegitimate brood. The firſt chemiſts deluded themſelves, and the world, with the hopes, of tranſmuting metals, and finding the grand elixir; and the enthuſiaſtic ſpirit of the *Germans* diſplayed itſelf, in the dreams and deſtructive purſuits of alchemy, and the fanciful extravagancies of the *Roſicrucians.*—In more modern times, religious fanaticiſm has re-appeared, and ſhewn, (though under forms leſs alarming and hideous than formerly) a powerful force of enthuſiaſm; in the trances, the abſurd raptures, and extatic viſions of the *Myſtics*, from *Jacob Behmen* down to *Emanuel Swedenborg*; not to ſpeak of the groſs but harmleſs follies of the *Moravian* brethren. In thoſe latter days, when the ſtudy of natural knowledge became univerſally prevalent, when the experiments in electricity were ſo much extended, and medicine and the economy of nature were cultivated, with extraordinary care, and produced a variety of ſurpriſing diſcoveries—here again the enthuſiaſtic temper of the Germans ſhewed itſelf, by engrafting on natural philoſophy and medicine the wild chimeras of animal magnetiſm, and the pernicious illuſions of *Meſmer.* Laſtly—in theſe days, of innovation, enquiry, and ſcepticiſm, it remained for *Germany* to originate the unaccountable and incoherent ſect of the *Illuminati*; a ſect of ſhreds and patches, a ſect compoſed of contradictions, which boldly attempts, to combine in one chaotic

chaotic maſs, the hardihood in deſpiſing eſtabliſhed forms and legal inſtitutions, and the innovating meddling ſpirit of the new philoſophy, the credulity and enthuſiaſtic bigotry, of antient religioniſts,* the dreams, the ſelf-deluſions, and ſupernatural pretenſions of *alchemiſts* and *Roſicrucians*, and the unintelligible jargon and incomprehenſible reveries of the myſtics.—And, ſuch is the catching power of enthuſiaſm, that the wildeſt and moſt penicious abſurdities of the *Germans* have rapidly ſpread, and found numerous adherents and followers, in the reſt of *Europe*.

It will not, I truſt, appear foreign from the purpoſe, if I ſhould add a few words, on the rage for *German* productions, and the German ſtyle, which is now ſo prevalent in *England*.—It is, indeed, extraordinary, that, in a country, which boaſts ſuch a high ſtate of refinement, this ſavage kind of writing ſhould find ſo many admirers, and partiſans; and form, as it were, a new ſchool of compoſition.—Muſt we attribute the phenomenon to a radical declenſion of taſte?—I ſhould be ſorry to think it—for the declenſion of taſte is the certain harbinger, of imbecility of mind, and the abſence of public ſpirit, and virtuous feeling. Taſte is only good ſenſe applied to the works of genius; a rectitude of ſentiment, a perception of beauty, and good order; and where theſe are wanting, one of the great foundations and ſafe-guards of moral

* See in Wraxall's Travels a ſurpriſing ſtory, of a conjuror, who profeſt to raiſe the dead.

moral conduct muſt fail.—Is it, that diſproportion, irregularity, and extravagance, have in them ſomewhat ſtriking, ſomething ſo different from the uſual practice of men in their works of art and genius, that they fill the croud, who are apt to hold cheap what they perpetually ſee, with an idea of novelty and greatneſs?—Thus, we ſee the *Chineſe* and *Gothic* architecture, have their admirers; and we find many modern ſtructures built in imitation of the *Chineſe* and *Gothic* ſtyle.—Are ſanguinary ſpectacles and tales of horror calculated, to obtain currency, and popularity, by exciting a powerful intereſt, on the ſame principle, that people throng to executions?—Or, is it, that the ſeeds of revolutionary principles, and the germs of innovation and anarchy, which are plentifully ſprinkled, through the German productions, ſeaſon them with an high reliſh, and recommend them to the palates of multitudes, who in thoſe times are deeply, though ſecretly, tinctured with the maxims of the new philoſophy, and love to find the image of their own thought reflected back on them from the ſtage?—Some obſervations on the preſent ſtate of ſociety and manners, may lead us to a ſolution of the problem.

Excessive luxury, and immoderate refinement, commonly degenerate into a voluptuous and ſenſual diſpoſition; which finds that the intellectual and purer pleaſures require too much exertion, and attention; and produces a ſort of ſupercilious apathy, the mortal foe of literature and genius. The public taſte becomes a ſort

a ſort of drowſy monſter; which muſt be waked from its ſlumbers, by the keeper, and expects to be fed with a competent proviſion of daily novelties; and, provided the meſs be freſh and in ſufficient quantity, the many-headed beaſt little regards the quality. Thus, luxury gradually depraves the taſte, and imbrutes the mind; until, by inſenſible approaches, it comes to loſe itſelf in the groſs ſenſuality, the childiſh predilections, and the rude barbariſm of the early, unlettered ages. The mind then requires to be rouſed, and ſtimulated, by ſomething extravagant, far-fetched, and uncommon. It no longer takes pleaſure in the chaſte and modeſt beauties of ſimple nature; the jaded fancy calls for the paint, the tinſel, the nakedneſs, and meretricious allurements of the venal wanton: *Horace* complains, that, even in his time,

——— Jam migravit ab aure voluptas
Omnis ad incertos oculos & gaudia vana.

How much more juſtly might the complaint be made at preſent!—In how great a degree does the theatre depend for ſupport on the mechaniſt, the painter, and the tailor!—Even when the crouded benches do condeſcend to liſten to the dialogue, what bribes them into attention? Are they attracted by the ſterling wit and humour, the diſplay of character, or the genuine pathos that reign in our admirable maſters of the Drama?—No—they are drawn together by the tranſlated traſh of ſome foreign novelty—they wait the appearance of a ghoſt or goblin—they hope to be rouſed from

their

their weary lethargy, into hyſterical laughter, or hyſterical tears, by the farcical, or the horrid.—They ſwallow, with gaping wonder, the eccentric flights, the profane rants, the *illuminated* morality, the bombaſtic diction, of imported patch-work, from their *German* favourites.

Another cauſe of the depravation of original taſte, and of the inundation of tranſlated works, which deluges the preſs and ſtage, may be found in the ſmall diſpoſition towards the encouragement of works of poetical genius and imagination, which is now but too obſervable. Poetry, and all works, whoſe prime and immediate object is amuſement, are undervalued by the falſe gravity, and faſtidious ſeverity of the preſent times; and deprived of the weight, to which they are really entitled, in the ſcale of public utility, and public eſtimation.—Mathematical diſcoveries, and the ſtudy of nature, are purſued with eagerneſs and ſucceſs; and bring due honour to their profeſſors.—Antiquarian reſearches have more than a due ſhare of public attention—Critical purſuits, and claſſic learning, are ſtill reſpected. The biographical part of hiſtory, as being connected with the favourite ſtudy of antiquities, preſerves her palm. But it is obvious, that the muſes of the country are conſigned to neglect and contempt. This may be owing to the predominant ſway of avarice and ambition, which gives a particular bias of the mind, and inclines it to ſlight and contemn every thing, which does not directly and rapidly tend to profit and advancement.

advancement. To theſe they find a more certain road, in the ſtudy of forenſic eloquence, and the details of finance, or political economy, than in the happieſt and nobleſt exertions of poetry. There are times, when ſpots prevail on the face of the ſun; and, for years together, ſadden the world, by rendering the ſeaſons backward, and diminiſhing his light, and heat, and chearing influence. May there not be ſomething, at preſent, in the political hemiſphere, which diſpoſes the mind to gloom and ſerioufneſs, and makes it unfit for the play of fancy, and works of imagination?—Certain it is, that the ſmall encouragement beſtowed on native and original works of genius tends to leſſen the dignity, and independence of poetry; to ſeparate the taſk of amuſing, from that of inſtructing the world; and to throw the latter office into the mean and griping hands, of a mob of tranſlators, little ſolicitous about their own fame, or the morals of the nation, provided they can find a compendious way to the pockets of the public.

THERE is certainly a cauſe, which lies deep and remote, and is not obſerved, with the attention it merits, which is ſilently operating, to produce a liſtleſs apathy, or a childiſh frivolity, in the ſpectators, and readers, and to diminiſh the numbers of both; and at the ſame time, contracts, with reſpect to the writer, his means of ſtudying the human heart, and obtaining a knowledge of life and manners. The middle order, the moſt moral, the beſt educated, and the moſt judicious part of the community, experiences

a gradual contraction and diminution, both in number and efficiency; and it is to be feared, the day is not far diſtant, which ſhall ſee it abſorbed, into the lower claſſes of the people. Wild and unbridled luxury, boundleſs expence; the enormous profits of trade; the ſudden and unexpected aggrandizement, or opulence of mean individuals, in conſequence, of this, and other cauſes; the amazing influx of Aſiatic wealth, and pomp, and ſoftneſs; the preſſure of taxes, great beyond all former example, and even beyond the ſuppoſed capabilities of finance; the exorbitant price of all the neceſſaries, not to ſpeak of the comforts of life;—theſe combining cauſes are bearing *Britain* forward, with rapidity, hourly accelerated, to the ſtate I mention.—Paſs a few years, and haply the yeomanry and gentry may diſappear; and leave only the crown, the court, an ariſtocracy, partly of nobility, partly of merchandize; and a wretched, ignorant, laborious peaſantry.—The one claſs, too great, too buſy, or too voluptuous, to frequent theatres, or waſte their time on books of amuſement; the other claſs, too poor and wretched, to purchaſe, or too groſs and uncouth, to reliſh intellectual pleaſures. I need not ſay, how unfavourable ſuch a change in ſociety, or even the approach to ſuch a change muſt prove, to elegant amuſement, polite letters, rational converſation, and an extended knowledge of human nature.

Thus much for the cauſes more particularly affecting the audience, and the readers. There are certain difficulties, which ſurround

round and perplex the writers of the preſent day, from which their predeceſſors were, in great meaſure, exempt. The intercouſe of ſociety is not, perhaps, as free, as eaſy, and as general, as in former times; the diſcriminating ſpace and interval between the different ranks is much widened; not by over-weening pride, but by exceſſive luxury, and a non-elective, an unaſſociating, and unaſſociable difference of manners. There may exiſt alſo ſome cauſes, peculiar to the times, which it is not neceſſary to particularize here, but which repreſs and narrow the converſation of man with man. The circulation of ideas, by writing and printing, has of late been conſiderably cramped, and confined, by laws impoſing reſtraints on the freedom of the preſs, and by the great advance which has taken place in the price of books and paper; not to ſpeak of a fooliſh and deſpicable luxury, which demands in every new publication ſuch a voluminous and expenſive pomp of typographical decoration, as precludes the middle claſs of readers, from their peruſal. Thus, the communication of ſentiments is abridged, and the means, and facilities, are retrenched, by which the knowledge of mankind is diffuſed, and the different ranks and orders of ſociety are enabled, to contemplate each other, as they really are. It is eaſy to conceive, what diſadvantages muſt redound from hence, to writers, whoſe ſubject matter ſhould lead them, to delineate the nice features of character, to anatomize the human heart, and repreſent the thoughts and actions of men. It is natural, that, perceiving all the impediments in their way, they ſhould

abandon the enterpriſe, in deſpair, and confine themſelves to the humbler, yet more popular walk, of tranſlation.

Omnia venalia Romæ—may be applied to *England*; and the rapacious and ſordid ſpirit appears as much in the republic of letters as in any other department. It is ſurpriſing how much the venality of literature, has been encreaſed, extended, and ſyſtematized! how it enlarges the margin, at the expence of the matter; and ſwells the volume, to the coſt of the reader! how it multiplies editions, by trifling alterations, and ſtoops to borrow a mean mechanical celebrity from the printer and engraver!—This mercenary ſpirit leads writers, to hurry their abortive miſbegotten productions into light; and what is worſe, to pay implicit deference, to the whims and caprice of the undiſcerning many; to the meteor fancies of the hour, in the choice of ſubjects, and in the manner of treating them. Thus, at one time, ſentimental Comedy,* which was naturalized, from the Dramas of *Fontenelle*, *Diderot*, and other French writers, was all-prevailing on the ſtage, and a moral ſentiment, a good-natured obſervation, or a ſtroke of the pathetic, was the ſureſt pledge of a clap, from the pit and galleries.—Then, ſucceeded the faſhion of writing at certain actors and actreſſes, which ſacrificed the whole regularity and ſtructure of the piece to the prominence of ſome particular character, which was contrived to

* Such as the Engliſh Merchant, Weſt Indian, Clandeſtine Marriage, &c. &c.

to keep the ſtage almoſt excluſively; while the author built his hopes of ſucceſs, not on the intrinſic merit of his performance, but on his felicity in hitting the fancy of the virtuoſo, or virtuoſa.—An undeſcribable ſtyle of good-humoured abſurdity, and pleaſant nonſenſe, then produced pieces conceived in the ſpirit of Chriſtmas gambols, whoſe only aim was to produce a horſe-laugh, and fill the pocket of the author,* and this, without putting the writer, or his audience, to the trouble of much thinking.—The public appetite began to be ſated with nonſenſe, which now required, to be reinforced by practical jokes, and corporal activity, and produced a run of Comedies full of buſtle and incident, ſtage-trick, ſtage ſituation, and pantomimical ſurpriſes. The German Drama, with its train of ghoſts, goblins, fiends, and enchanters, is the reigning ſultana of the hour, and no Tragedy, Comedy, Opera, or Farce, may hope to ſucceed, at preſent, without a ſpectre, or apparition, a trap door, an haunted tower, or an enchanted chamber.

Nor ſhould we be ſurpriſed at all this. The Drama is at the mercy of managers, always intereſted, often undiſcerning, and ignorant. The writer for the ſtage, regardleſs of reputation, if he looks for emolument, muſt be an humble drudge, a verſatile ſlave, anxiouſly watching the fluctuations of whim, the vagrant caprices of the moment. To ſuch creatures of a third night and a green-room,

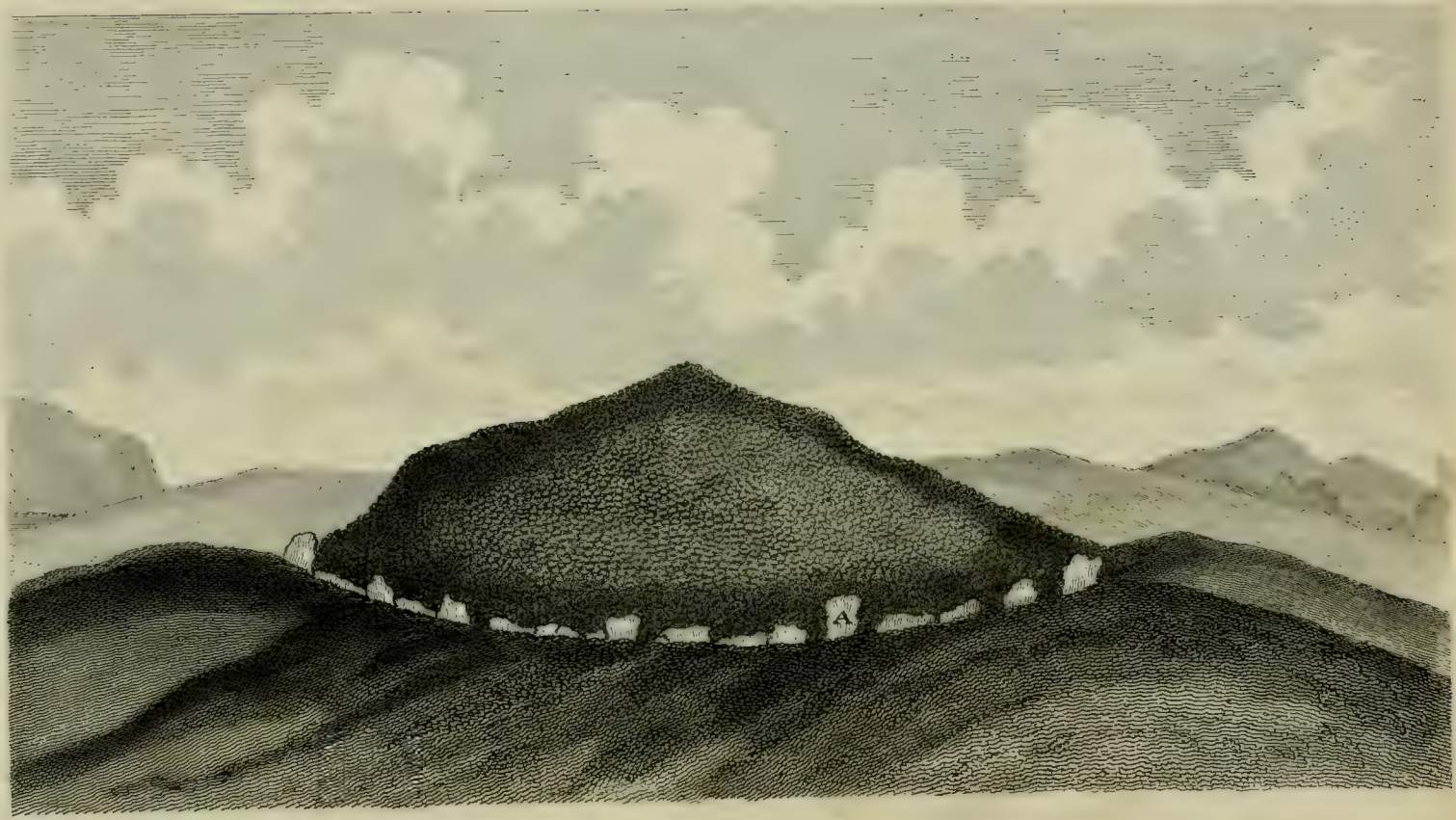
* O'Keeffe, *for inſtance.*

room, tranſlation is a much eaſier taſk, than original compoſition. Many are capable of doing a *German* play into *Engliſh*, of abridging, and fitting it for the ſtage; who are unequal to the taſk, of inventing a fable, and a ſet of characters, and arranging the incidents of an entire play. If we ſuppoſe, in managers, writers, and audience, a combination of bad taſte, the very eccentricities and extravagance of the German Drama will be apt to faſcinate ſuch judges. They abound in extravagant and over-marked characters; in furious burſts of paſſion, and outrageous rants; thus are they adapted to the ambition, and affectation, of performers, who delight in over-acting, from the ſwelling Tragedian, who ſplits *the ears of the groundlings*, to the maſter of grimace, who ſets the upper gallery in a roar. There is ſomething too in the German wildneſs and exceſs, which is well fitted, to pleaſe the unformed taſte of the vulgar part of an audience (though, at preſent, perhaps, the humbleſt part of an audience is not the moſt vulgar in its taſte) with ſomething monſtrous and glaring. The truth of colouring, and modeſty of nature, are hard to hit, and difficult to repreſent. They require a correct taſte, and formed judgment, to feel them properly. Extravagance and exceſs are more eaſy, in the repreſentation, and more apt to draw repeated plaudits, from an injudicious audience, by the force of ranting ſcenes, and bravura ſpeeches.

After all, it may not be chimerical to ſuppoſe, that the general reception of the *German* writings, the univerſal prevalence of the

the German taſte, and the love of the wild and gloomy, are not to be accounted for, from ordinary cauſes; and have in them more weight and importance, than are uſually attached to mere matters of taſte and criticiſm. May not theſe be among the elements, of feveriſh agitation, and mighty change, afloat, by the permiſſion of Providence, for purpoſes, to us inſcrutable, in the moral ſyſtem? May not this revolution in taſte be a prelude to other revolutions; a ſmall ſkirt of the cloud, *like a man's hand*, uſhering in the blackening tempeſt? Are not the German writings calculated, to generate, in both ſexes, a ferocious hardihood, and independence of mind; a dangerous contempt of eſtabliſhed forms; a promptitude, to ſuffer and to dare; an enthuſiaſm of character, fitting them for ſeaſons of energy, of exertions, of privations, dangers, and calamities? It is natural, for human blindneſs and inattention, to overlook the inſtruments, and operations, by which Providence prepares and faſhions great and ſurpriſing events. It is the folly of man, to aſcribe too little weight and importance to moral cauſes; while, it is the courſe of Providence (as it were, on purpoſe, to humble human pride,) to act, by ſeemingly minute and inefficient cauſes. Who knows, then, but this preternatural appetite for the irregular, the indecorous, the boiſterous, the ſanguinary, and the terrific, may be the precurſor of ſome ſtrange, moral, or political convulſion?

ANTIQUITIES.



To front P. 3 in Antiq.

Some ACCOUNT *of the* VICARS CAIRN, *in the County of* ARMAGH; *communicated to the* COMMITTEE *of* ANTIQUITIES *in two* LETTERS, *one from* DR. BROWNE, *Senior Fellow of Trinity College, Dublin; the other from the* REV. JOHN YOUNG, *Curate of* MULLABRACK.

DR. BROWNE, in his letter, dated the 19th day of December, 1799, ſays, That notwithſtanding all that has been written, (by very learned men,) of the Ogham character, and ſome modern teſtimonies reſpecting its exiſtence, he was extremely incredulous as to any monuments being actually extant on which it could be found, and diſpoſed to think that literary enthuſiaſm had miſtaken natural furrows on the ſtone for engraved characters. But that having ſatisfied himſelf that he was in error, he thought it a duty to the Academy, to mention a monument of the kind which had come under his obſervation. Being at Armagh in the

Read, Dec. 21, 1799.

 year

year 1797, the Lord Primate,* fond of every ſpecies of literature and not unacquainted with any, told him it was generally ſaid, that on a high hill, about three miles from the town, an Ogham inſcription was to be found. Dr. Browne rode up to the top of this hill and found on its ſummit a ſmall conical mount or rather heap of ſmall ſtones, ſurrounded by a regular circle of large ones upright and about two feet and an half high. The country people immediately directed him to one of thoſe ſtones on which they ſaid the inſcription was; the day being very windy and the ſtone low, he was forced to lie down on his ſide to copy it, which he did as well as he was able. At firſt view he ſays he was not ſure that the indentures in the ſtone were not natural, but on obſerving them carefully and their regularity, and comparing them with the natural impreſſions which were irregularly indented in the other ſtones and on ſome part of this, he convinced himſelf beyond a doubt, that they were artificial, and this is ſtrongly confirmed by the tradition of the country.

Dr. Browne was again at Armagh in September 1799, and again viſited the Vicars Cairn with a friend. Not inſtantly recollecting the particular ſtone, he aſked a peaſant who was paſſing with a load of turf not far off, if he knew the ſtone upon which letters were ſaid to be written; he immediately aſked, if it

* Doctor Newcome.

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it was the written ſtone that was ſought? and ſhewed it to him without heſitation, adding, however, at the ſame time, that he did not believe the marks repreſented letters. On this Dr. Browne took the opinion of his friend in confirmation of his own, and they both agreed, without the ſmalleſt doubt, that the characters or lines were artificial, and indented by ſome ſharp inſtrument. He then with a rule and pair of compaſſes took a copy of them, with which he does not trouble the Academy, as he has ſince ſeen a more accurate one taken by the Rev. Mr. Young. In going round the hill marks were perceived of what appeared the mouth or entrance of a cave, of which they endeavoured to clear the entrance, but found it much beyond their ability; yet they obſerved enough to impreſs them with a ſtrong perſuaſion, that the hillock is excavated, the entrance being very like that at New Grange. Another reſemblance is in the ſurrounding circle of upright ſtones; which, (together with the want of a ditch or foſs,) always diſtinguiſhes ſuch tumuli, though they are by Dr. T. Molyneux, called Daniſh mounts, from the forts and mounts uſually called raths*, which word alſo, as he conceives, means a place of judgment. On their return to the written ſtone, by taking up the earth, they diſcovered more indented lines towards the bottom of its interior face, longer than any they had ſeen; but during their abſence, the peaſant, had, ſtolen their ruler, which

* It is remarkable that *Rath*, in the German language, ſignifies counſel or advice, and Der Rath, a judgment ſeat, or ſenate or council.

which happened to be of ivory, and prevented further copying of theſe curious marks. Dr. Browne obſerves, that he could not ſatisfactorily trace any tranſverſe horizontal line, croſſing the others, ſuch as is uſually delineated in the Collectanea and elſewhere; he thought he could in ſome places obſerve ſuch a line, but could not venture to be poſitive.

THERE is, he ſays, within ſeven miles of Dublin, a circular range of ſtones, with a ſtone elbow-ſeat inthe middle of them, evidently the place of the judge; it crowns the top of a hillock, and nothing can more evidently ſpeak its purpoſe than its appearance. It is on the deſcent of the Dalkey hills, yet though ſo near, little known and ſeldom viſited. The inhabitants of Ireland, he obſerves, ſeem in general not ſufficiently attentive to the antiquities ſo thickly diffuſed through the kingdom.

MR. YOUNG, in his account, dated October 14th 1799, ſays, The immenſe heap of ſtones, called the Vicars Cairn, lies to the ſouth-eaſt, and at the diſtance of four miles from Armagh, in the pariſh of Mullabrack, ſeems to be much diminiſhed from its original ſize. During the memory of the oldeſt men in the neighbourhood, it has been uſed as the public quarry for the repair of roads; yet whatever the object of the firſt builders was, ſeems now

now to be entirely unknown, but from its extent it muſt have been a work of conſiderable labour, time and expenſe.—The area is circular, forty-four yards in diameter. In the amaſſing of this heap, there has not been obſerved any kind of regularity, except in the ſtones of the circumference, theſe (from the preſent remains) ſeem to have been placed cloſe to each other, in order to contain the ſmaller ſtones of which the Cairn is compoſed. Thoſe that remain perfect are one yard each above the ſurface of the ground, and ſuppoſed to be as much below it; they muſt have been all (when firſt placed there) equal, or as nearly ſo, as ſtones in their natural ſtate could be procured.—The moſt perfect ſtone A, which is that bearing the Ogham characters, of which a drawing is given, ſtands declined in an angle of twenty-five degrees from the perpendicular; this declination was intended the better to contain the ſmaller ſtones.—After the firſt external row, all regularity was laid aſide, the ſtones were thrown together promiſcuouſly and of different ſizes; they ſeem to be in general the common ſtones collected off the fields. Perhaps the delineation of the Ogham characters, with ſome circumſtances, which have lately appeared, relative to the inſide of this maſs, may aſſiſt the antiquary in his reſearch, and enable him to know, at this very remote period, in ſome degree, what the intention of the original builders had been.

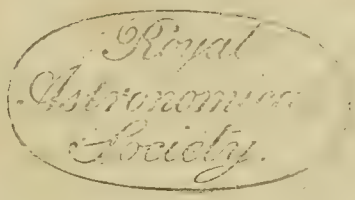
In the year 1785, John M'Carrol, proprietor of the ground on which the Cairn ſtands, having penetrated the weſt-ſide farther than

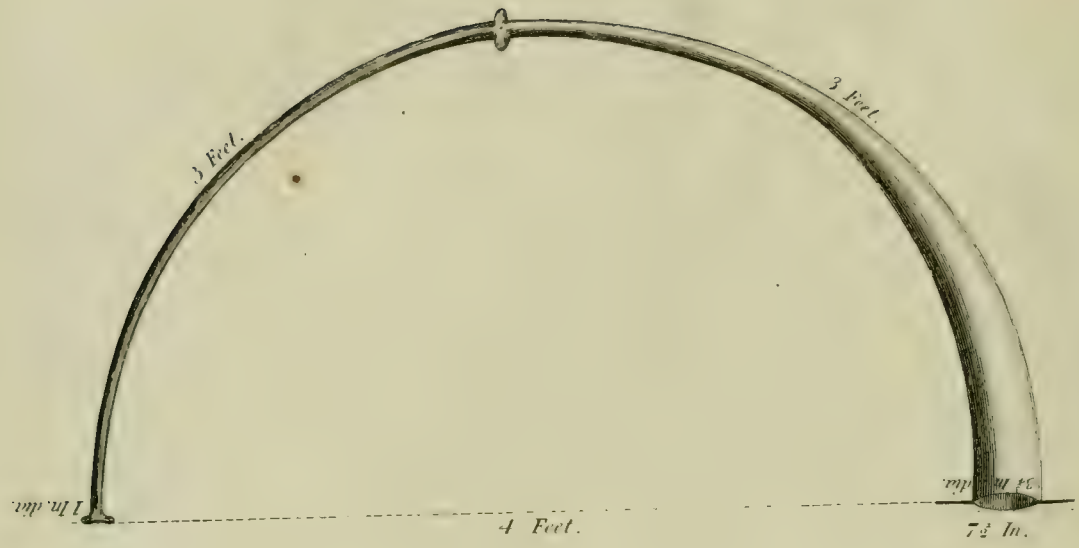
than it had been opened before, (in ſearch of large ſtones in order to build a lime-kiln) came on a door-caſe; it appeared perfectly ſound, but when touched fell into duſt; it had been ſupported there by the ſtones piled up on either ſide. The hopes of being rewarded by ſome hidden treaſure (which is the general belief of the country) induced him to remove more ſtones than he had at firſt intended; he at length came on a wall ſituated eaſt and weſt on either ſide of the door-caſe; the ſtones were placed together without any kind of cement, and ſeemed to have been regularly prepared for the building.—After labouring for ſeveral days, and having traced the wall for ten feet, but without meeting with any thing to reward his trouble, he left off the work.—Since that time the inſide of this heap has remained unexplored.—It is held in great veneration by many of thoſe who live near it, who account it impious to carry off any of the ſtones.—If it ever was frequented by any religious ſect, on any particular day, for the purpoſes of worſhip, the record of it is totally loſt.—The very zealous Roman Catholics never paſs or repaſs without bringing a ſtone and throwing it on the Cairn to add to the common heap.—They tell many ſtories of the misfortunes that have happened to thoſe who were daring enough to remove any of the ſtones of this ſacred pile; and what tends to confirm them in theſe fancies is, that ſome men living near the place, having ventured to uſe theſe ſtones for the common purpoſe of building, have ſince that time been viſited by

different

different diſeaſes, which they look upon as the juſt conſequence of their impiety.

The ground on which this Cairn ſtands is much higher than any other part of the country within many miles of it. From this very elevated ſituation, on a fine day, the ſpectator is gratified with a diſtinct view of ſeven different counties, viz. Armagh, Monaghan, Tyrone, Antrim, Derry, Down, and Louth, and many lakes of different ſizes, adding much to the beauty of the ſcene.

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An ACCOUNT *of ſome* ANCIENT TRUMPETS, *dug up in a* BOG *near* ARMAGH. *By* ARTHUR BROWNE, *Eſq. Senior Fellow of Trinity College, Dublin.*

Read Dec. 21, 1800.

BEING at Armagh in September laſt, I was caſually informed that one of the tenants of his grace the Primate, by name Pooler, had in his poſſeſſion ſome ancient trumpets not long ſince dug out of a neighbouring bog. I went to his houſe, which is about a mile from the town, and there ſaw two of them, and afterwards a third at the houſe of an artiſt in Armagh, where it had been ſent to be repaired;—a drawing of the moſt perfect is hereunto annexed.

MR. POOLER informed me, that four of them had been dug up at the ſame time, and nearly in the ſame place; and that the tradition of the place is, that a mighty battle was there once fought, and that ſome king of Ulſter had his palace not far diſtant; but when, or between whom the combat was reported to have been, he could not give me any information.

The colour of the trumpet is that of braſs—the ſmell alſo that of braſs; for they left on the fingers a very diſagreeable odour not eaſily or quickly removed.

Their antiquity appears from the peculiarity of the metallic compoſition, which is different from any of the modern times; and from the parts being joined entirely by rivets, evidently before the art of ſoldering was known; and from the ornaments about the entire, I think it is plain that they muſt have appertained to a nation not in a ſtate of barbariſm. Not being an antiquarian, I do not venture to ſay more. When I ſaw them they were not ſufficiently in repair or tight to produce ſound; but one of them had been made by an artiſt, in the vulgar expreſſion, wind-tight, and ſounded by a trumpeter belonging to the 23d regiment of dragoons, and, as I was informed, produced a tremendous ſound, which could be heard for miles; by the deſcription I ſhould conceive ſomewhat reſembling that moſt terrific of all ſounds which I have heard, according to my fancy, that of the Oriental Gong.

The form of theſe inſtruments evidently ranks rather under the Cornu than the Tuba; thoſe deſcribed and depicted by Doctor Molyneux, in his Natural Hiſtory of Ireland, and by a learned member and great antiquarian, General Vallancey, in the fourth volume of the Collectanea, are of an entirely different conſtruction. I conjecture this to be the Dudag or Skeh Trumpet of braſs mentioned by General Vallancey.